MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY

Vol. 104 No. 5

MEETING OF 1944 SEPTEMBER 8

Professor E. A. Milne, President, in the Chair

The election by the Council of the following Fellows was duly confirmed:—

William Douglas Anderson, Altair, 40 Pacific Street, Roslyn, Dunedin, N.W. 1, New Zealand;
Derek Martin Butcher, F.R.Met.Soc., 21 Hillcrest Road, London, W. 3;
James Clendinning, O.B.E., B.Sc.(Eng.), A.M.Inst.C.E., 23 Westfield Park, Redland, Bristol, 6;
Henry Richard Didcock, 91 Hugh Street, Castleford, Yorkshire; and
Robert S. J. Edwards, The Vicarage, Stanton, Shropshire.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

Frederick Ronald Curtis, B.Sc., 6 Hartside Gardens, Jesmond, Newcastle-upon-Tyne, 2 (proposed by I. F. C. Carr Gregg);
Norman John Harris, The Priory, Priory Park, Blackheath, London, S.E. 3 (proposed by M. C. Johnson); and
Russell F. Moore, Headquarters, 66th Medical Group, A.P.O. 403, U.S. Army (proposed by R. W. Cartmel).

The President paid the following tribute to Sir Ralph Fowler (the Fellows standing):—

"The Society has lost by untimely death one of its most eminent Fellows. Sir Ralph Fowler, though not primarily an astronomer, had served on our Council, and had written a number of important astrophysical papers in our *Monthly Notices*. At an early stage in his career he became interested in the Saha high temperature dissociation theory, and your President had the privilege of collaborating with him in two papers on the method of determining the pressures in stellar atmospheres by means of the temperature of maximum intensity of 'subordinate' absorption lines in the stellar spectral sequence. Later Fowler became interested in the statistical mechanics of degenerate gases, and in

a fundamental paper entitled 'Dense Matter' showed that the ionized material at high density in the interiors of white dwarf stars must be in a state of degeneracy. Later still he contributed papers of great mathematical beauty on the theory of solutions of Emden's differential equation, which is so important in the theory of the internal structure of stars. These three topics—stellar atmospheres, white dwarf stars, Emden's equation—serve to illustrate Fowler's amazing versatility; he had the power of rapidly mastering a subject down to its intimate details, at very short notice, and making effective contributions to it as if from one who had made it a life-long study. He was accomplished both as a pure mathematician and as a mathematical physicist, to both of which fields his book *Statistical Mechanics* is a permanent adornment.

"He was born on 1889 January 17, educated at Winchester and Trinity College, Cambridge, of which college he became a Fellow and subsequently Lecturer. He was elected Plummer Professor of Applied Mathematics in the University of Cambridge in 1932. He was appointed Director of the National Physical Laboratory in 1938, but an illness, which he fought manfully, prevented his taking up the appointment. He died on 1944 July 28, at the early age of fifty-five, after a life crowded with every kind of mental and physical activity. British Science will long mourn him."

Sixty-seven presents were announced as having been received since the last ordinary meeting, including:—

C. W. C. Barlow and G. H. Bryan, *Elementary Mathematical Astronomy*. Revised by Sir Harold Spencer Jones. Fifth edition (presented by the publishers);
Bart J. Bok and F. W. Wright, *Basic Marine Navigation* (presented by the authors);
H.M. Nautical Almanac Office, *Apparent Places of Fundamental Stars, 1945* (presented by H.M. Stationery Office);
Sir Harold Spencer Jones, *Metals in the Stars* (presented by the author);
Roy K. Marshall, *Things that Fall from the Sky: Meteors and Meteorites* (presented by the author); and
W. H. van den Bos and W. S. Finsen, *Handbook on the Union Observatory Universal Sun Compass* (presented by Dr. W. H. van den Bos).

The President announced that, owing to the cancellation of the July meeting, the George Darwin Lecture would be delivered on 1944 October 13 by Professor J. Proudman on "The Tides of the Atlantic Ocean".

MEETING OF 1944 OCTOBER 13

Professor E. A. Milne, President, in the Chair

The election by the Council of the following Fellows was duly confirmed:—

Harold Whitaker Browne, 24 Tamarisk Avenue, Reading, Berkshire;
Sydney Gerald Carpenter, 19 Farm Avenue, Streatham, London, S.W. 16;
Charles Donovon Grimwade, Capella, Parkway, Trentham, Staffordshire;
John McLean Hildt, jun., U.S.N. 803, London Fleet, P.O.;
Renouf William Arnold Johns, The Mascot, Westward Ho!, Devon;
Philip Stevenson Laurie, 115 High Street, Chislehurst, Kent; and
John Gwynne Lloyd, B.Sc., 43 Upper Cimla Road, Neath, Glamorganshire.

The following candidates were proposed for election as Fellows of the Society, the names of the proposers from personal knowledge being appended:—

Ralph Stuart Atkinson, Beechwood, White Hill Avenue, Barnsley, Yorkshire (proposed by B. Burrell);

Richard Beetham, M.A., 14 The Corners, Cleveleys, Blackpool, Lancashire (proposed by R. Stoneley);

Arthur Bernard Garnet Drabble, 116 Raleigh Street, Nottingham (proposed by R. R. S. Cox);

Gordon Glynne-Walton, White Lea Grange, Batley, Yorkshire (proposed by A. R. Hinks);

Dudley Stewart Perfect, M.A., D.Phil., Royal Observatory, Greenwich, London, S.E. 10 (proposed by H. Spencer Jones); and

Michael George Sumner, Woodbury, West Road, Bridport, Dorset (proposed by D. H. Sadler).

Twenty-six presents were announced as having been received since the last ordinary meeting.

Professor Proudman delivered the George Darwin Lecture (see p. 244), taking as his subject "The Tides of the Atlantic Ocean".

THE TIDES OF THE ATLANTIC OCEAN

(*George Darwin Lecture, delivered by Professor J. Proudman, F.R.S., on* 1944 *October* 13)

It is appropriate, I think, that I should lecture on a subject connected with the tides of the ocean, for Sir George Darwin himself gave a great deal of his attention to this subject. From about 1882 until the end of his life in 1912, he was generally regarded as the greatest living authority on ocean tides.

I shall be mainly concerned with the discovery of the distribution of the tides over the open Atlantic Ocean, by the application of the principles of dynamics. This will involve a large measure of dynamical explanation of the tides as they have been observed on the coasts of the continents and islands. Owing to the great difficulty of measuring tides in mid-ocean, it is at present only by theoretical methods that we can expect to discover their distribution over the open oceans. A method depending on the tidal analysis of barometric observations has been put forward by E. Barkow (1911) and J. Bartels (1926), but has not been successfully employed, and to me the difficulties seem very great. Up to the present time the leading features of the tides have not been determined in any really convincing way. I propose to give a progress-report on an attempt which I have recently been making.

It was Newton who laid down the fundamental principles on which the tides are to be explained, and thus reduced investigations to problems in mathematics. A great deal of attention has been paid to these problems, some of it by the ablest of mathematicians, including Laplace and Poincaré. Much progress has been made in the determination of the tides in ideal geometrically simple basins, notably in recent years by A. T. Doodson and G. R. Goldsbrough. I shall show that some progress has been made with the actual basin of the Atlantic, but practically none has been made with the basins of the Pacific and Indian Oceans.

A question often asked is: Why are the tides of the Mediterranean so small? The answer is: The tides of the Mediterranean are just about of the magnitude that the simplest dynamical theory indicates. The much more interesting question is: Why are the tides of the Atlantic so large?

In order to introduce a fair amount of precision into the argument, I am going to restrict myself very largely to the principal harmonic constituent of the tides. This is the oscillatory motion with a period of 12 hours 25 minutes which is commonly denoted by M_2. For most places in the Atlantic it may be regarded as the average tide. In this constituent only the generating forces of the Moon are taken into account, and the Moon may be taken to remain in the plane of the Earth's equator and at a fixed distance from the Earth.

From the results of the analysis of observations the amplitude of this constituent has been evaluated at a large number of stations both on the continental coasts and on islands. A very convenient standard for the order of magnitude is provided by the equilibrium-form of the tides. This equilibrium-form gives what the tides would be if the water could immediately take up the position corresponding to the tide-generating forces of the Moon. Now the amplitude of the equilibrium-form of the tides is a function only of latitude and is given by $25 \cos^2 \phi$ cm., ϕ denoting the latitude, the mean value of which along a meridian is 12·5 cm. It is often not realised that the tide-generating forces are so small. Taking the mean amplitude of the equilibrium-form as a standard, there is at Brest a 16-fold magnification, at Boston, Mass., an 11-fold magnification, and at the oceanic islands a magnification of 4-, 3-, and 2-fold. This degree of magnification indicates that there is some form of resonance in the basin of the Atlantic.

The longer curves of Fig. 1 show the distribution of the tidal elevation along the continental coasts, including a line just outside the West Indies. The full lines show

the Greenwich lunar time of high water and the broken lines the amplitude. The data for this figure have been obtained by taking mean values along stretches of the open coasts, each covering 5° of latitude. We notice that there is a progression northwards

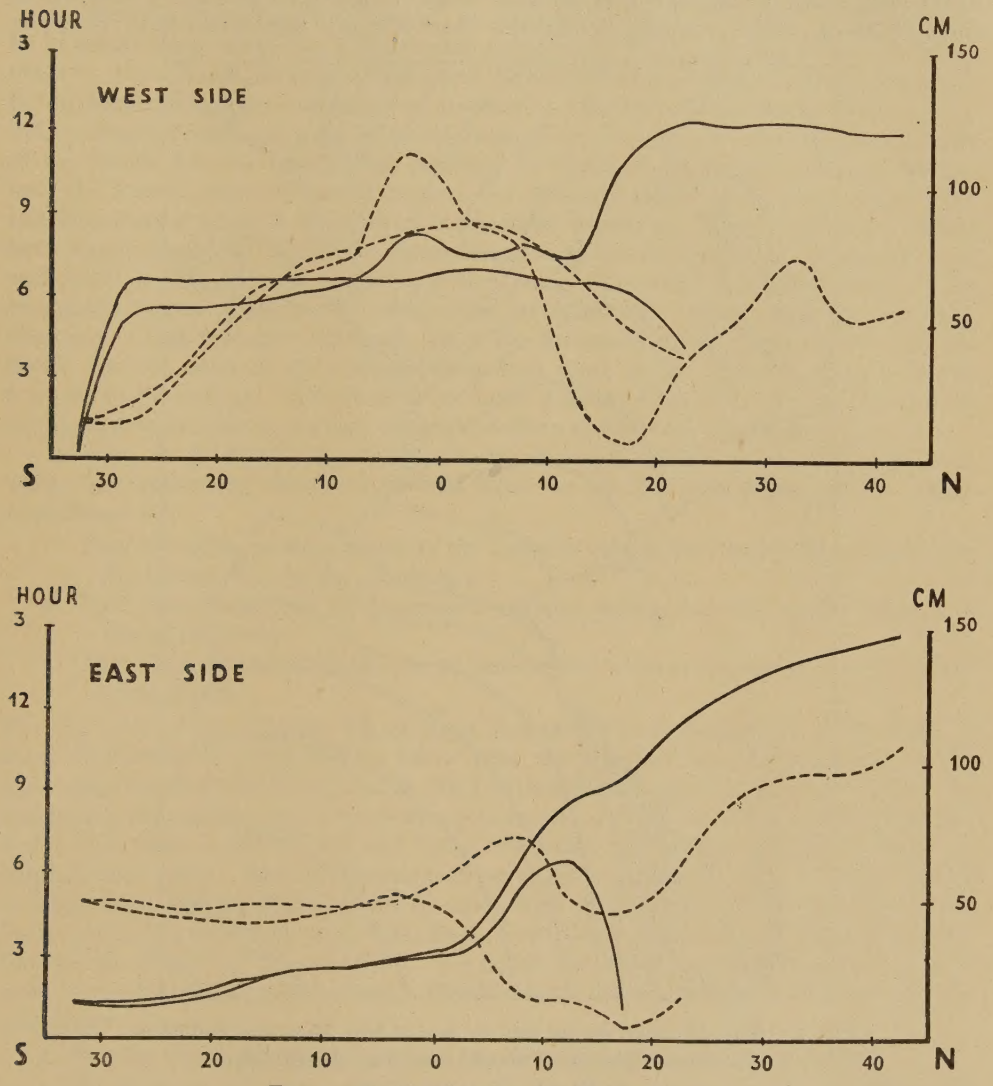

FIG. 1.—*Coastal elevations for M_2 constituent.*
Observation (longer lines) and synthesis (shorter lines).
Full lines give Greenwich lunar hour of high water.
Broken lines give amplitude in cm.

up both coasts of the South Atlantic and along the eastern coast of the North Atlantic. On the west side, at about 32° S. and 17° N. the range is very small and the time changes very rapidly; from about 2° S. to 14° N. and again along the U.S.A. from 22° N. to 42°·5 N. the times change very little, the hours being 7 and 12 respectively. Our problem is to find the theoretical distribution of the tides over the ocean which agrees with these coastal observations, and then we can check the theory by comparing the theoretical distributions with observations on the islands.

I shall now give a short historical review of the attempts that have been made to

account for the chief features of the semi-diurnal tides of the Atlantic. In 1833, William Whewell published the first *co-tidal charts*, and Fig. 2 shows his chart for the Atlantic.* The idea of a co-tidal chart is that along each line high water occurs at the same time; in Whewell's case the numbers give the solar times of high water on days of New Moon and Full Moon. The reasoning behind this chart is largely non-dynamical. Only the

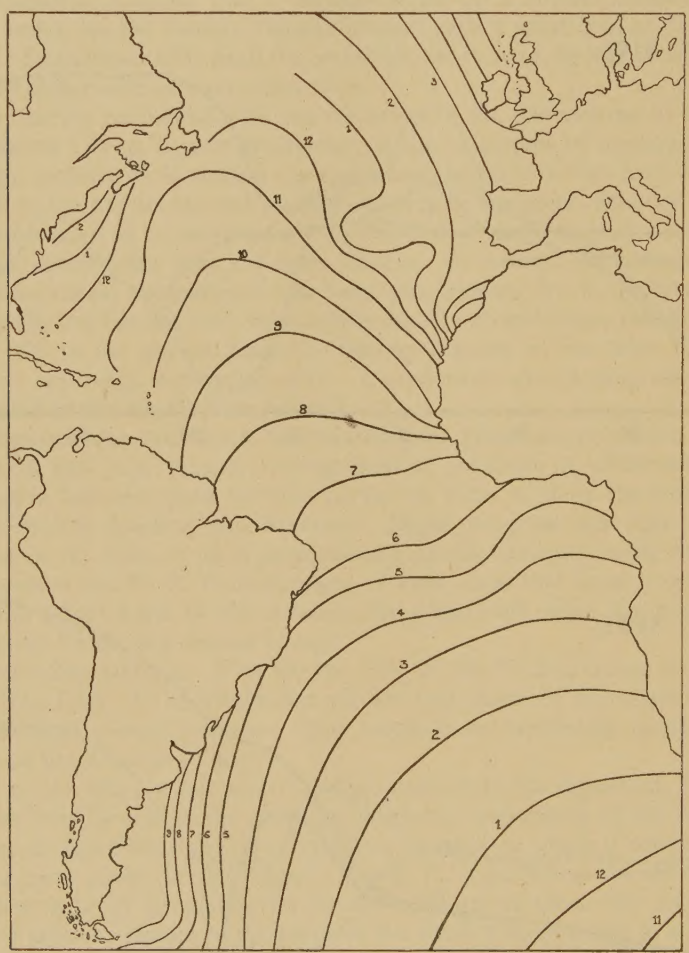

FIG. 2.—*Whewell's chart for the tides on days of New Moon and Full Moon. The numbers give the Greenwich solar hour of high water.*

forward curvature of the co-tidal lines is derived from dynamics, and this derivation is wrong. The lines just connect up the various points on the coasts at which high water has been observed to occur at the times indicated. In order to produce his charts Whewell organised an extensive series of observations. On the west, he got the simultaneity along the coast of the U.S.A., but he did not get the crowdings of co-tidal lines near 32° S. and 17° N. This particular chart led to the hypothesis that the tides of the Atlantic are waves derived from the Southern and Pacific Oceans. In 1842, Sir George Airy stated that he could not accept this hypothesis, and by 1848 Whewell himself had very largely abandoned it; but it lived for a long time in textbooks. Whewell's hypothesis raised the question as to whether the tides of the Atlantic are generated mainly within or mainly without the basin of the Atlantic itself. According to the hypothesis

* W. Whewell, *Phil. Trans. Roy. Soc.*, 1833, 147.

they would be generated mainly without the basin of the Atlantic. The question requires more precise statement, and when this is done it is seen that the problem of obtaining a complete answer is more difficult than that which I have already formulated. Of course, such a complete answer has not yet been given.

In 1874, William Ferrel calculated, on dynamical principles, what the tides would be in canals along parallels of latitude and stretching from one side of the Atlantic to the other.* For a canal between Ireland and Newfoundland he thought that there might be an approach to the conditions of resonance. He considered that if there were a dyke extending from the Cape of Good Hope to the coast of South America, the tides of the North Atlantic would most probably be very nearly the same as they actually are. In Ferrel's case there were no meridian components of tidal current, but I have considered zonal bands in which there is no lateral constraint. I have taken into account both longitudinal and transverse components of generating force, the variations over each band in the direction of the meridian being determined by the variation of the generating forces. Some of the amplitudes are quite large; larger than those actually observed. This indicates that large tides are generated in the basin of the Atlantic itself. On the coast of the United States high water in these bands occurs at about 6 hours in lunar time. But there is so much discontinuity between the tides in two contiguous bands that it is a very difficult problem to find the connecting links.

In 1900, R. A. Harris introduced a series of hypotheses on the basis of which, in 1904, he constructed charts of co-tidal lines for all the oceans and seas.† These hypotheses are:

1. That the tides are dominated by the forms of simple free oscillations which have the period of 12 hours 25 minutes.
2. That these forms can be determined without paying attention to the absence of lateral constraint.
3. That these free oscillations can be determined without reference to the rotation of the Earth.

For the tides of the Atlantic, Harris used four simple free oscillations. The times of these oscillations are supposed to follow from the times of the various phases of the generating forces of the Moon, but in this I believe Harris was three hours in error. In combining the oscillations of overlapping areas, we see that at the intersection of two nodal lines there is still no rise and fall of the water, and in this way we arrive at an amphidromic point. Also, the openings cause progressive waves. Fig. 3 shows Harris's co-tidal chart, and the times on this do agree with observations. The nodal lines have been replaced by crowdings of co-tidal lines ; in particular there are crowdings of co-tidal lines on the west at 32° S. and 17° N. We notice that there is a progression from east to west across the north of the ocean. Harris's work may be subjected to some serious criticisms:

1. For the South Atlantic he neglects the resonance of transverse oscillations.
2. He gives no adequate consideration of the effect of the water in neighbouring regions.
3. He gives no adequate consideration of the dynamical effects of the Earth's rotation.
4. His work is very largely only qualitative; he gives no absolute amplitudes of tide.
5. His times do not follow from the generating forces.

His chart was not accepted by Sir George Darwin.

In 1910, Poincaré discussed the energy implications of progressive waves. An influx of energy into any region must be balanced by:

1. The dissipation of energy by the friction of tidal currents on the bottom of the ocean and connected seas.

* W. Ferrel, *Tidal Researches*, 237, 1874. † R. A. Harris, *Manual of Tides*, Part IVB, 1904.

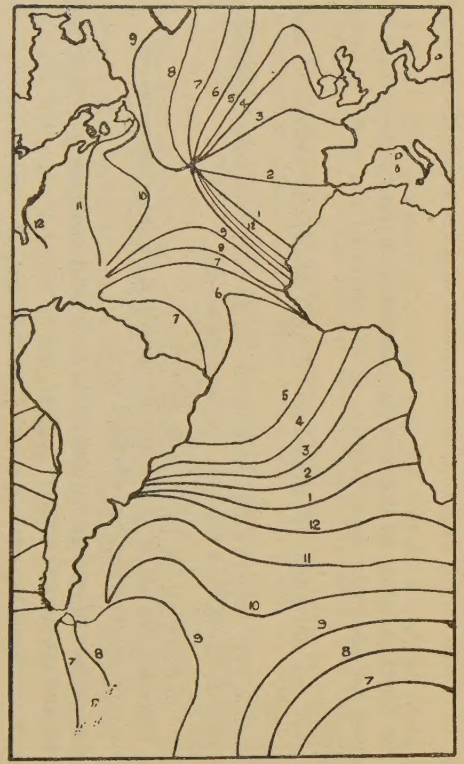

FIG. 3.—*Harris's chart for the M_2 constituent.*
The numbers give the Greenwich lunar hour of high water.

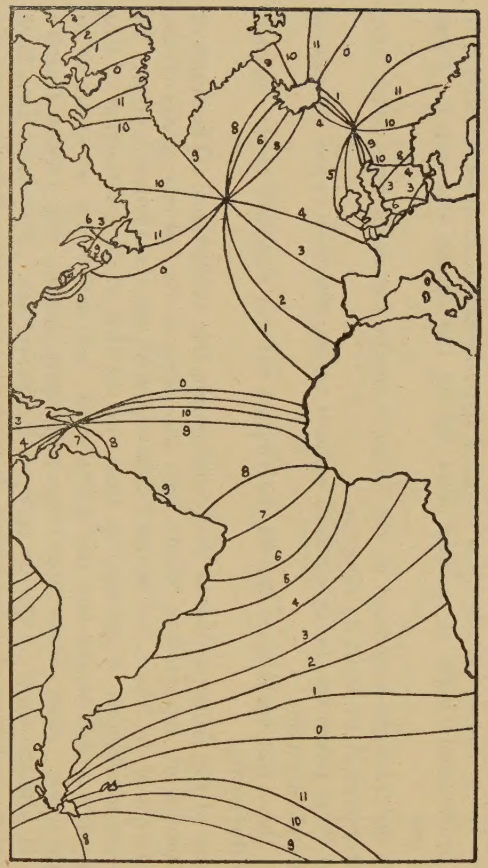

FIG. 4.—*Sterneck's chart for the tides on days of New Moon and Full Moon.*
The numbers give the Greenwich time of high water in hours which are one-twelfth of the period.

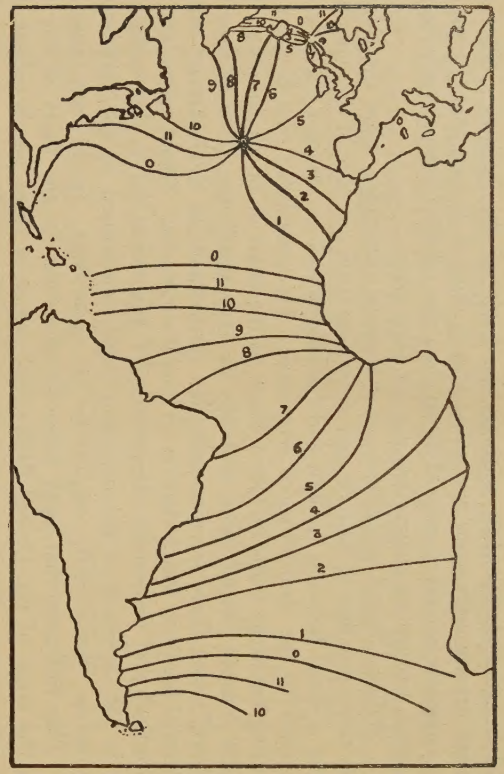

FIG. 5.—*Defant's chart for the semidiurnal tides.*
The numbers give the Greenwich hour of high water.

2. The work done against the generating forces.
3. The flow of energy out of the region.

In applying this principle to the Arctic seas, Poincaré assumed No. 2 to be negligible on account of the smallness of the generating forces in the Arctic, and No. 3 to be negligible on account of the smallness of Bering Strait. He assumed that No. 1 is always negligible. He thus deduced that the Arctic tides do not consist of a wave progressing away from the Atlantic. Incidentally, it would also follow that the energy passing into the Atlantic from the Southern Ocean must be given up to the tide-generating forces acting on the water of the Atlantic. But friction is not negligible, as was shown in 1918 by G. I. Taylor; and the progression of co-tidal lines does not necessarily involve a progressing of energy. For the M_2 constituent in the northern hemisphere and for a northerly progression of co-tidal lines, if the range increases towards the east sufficiently rapidly then the current at the time of high water may be directed towards the south, and in this case there will be a mean progression of energy towards the south.

In 1920, H. Jeffreys continued Taylor's work on the dissipation of energy in shallow seas, and in 1923 he showed that this friction must have very important effects on the distribution of the tides. There is another strong argument for the important effects of friction on the distribution of the tides, but for this we have to go beyond M_2 and consider also the corresponding solar harmonic constituent, S_2. One of the most striking facts about the actual tides is that, almost universally on the open coasts of the ocean, spring tides occur sometime *after* New and Full Moon. This can be accounted for by the dissipation of energy in marginal seas. It was in fact one of the points in favour of Whewell's hypothesis, for the S_2 constituent having a shorter free wave-length than the M_2 constituent, there would be an increasing lag in the time of S_2 high water behind the time of M_2 high water as we proceed northward up the ocean. But apart from this hypothesis it is clear that marginal dissipation must form a part of any adequate dynamical explanation of the observed tides.

In 1920, Sterneck published co-tidal charts for all the oceans.* These charts are not based on dynamical principles, but only on observations. He took all the observations available, resolved them into two phases differing by a quarter period, and then estimated the positions of lines of zero elevation in each phase. The intersection of a line of zero elevation in one phase with a line of zero elevation in the other phase gives an amphidromic point in the combination. By joining these amphidromic points to various points on the coasts he produced his co-tidal charts. For the semidiurnal tides of the Atlantic, his co-tidal chart, Fig. 4, shows the same general features as that of Harris, but the co-tidal lines for the North Atlantic have a more natural shape. He has the crowding of co-tidal lines on the west near 17° N. but not near 32° S.

In 1924, A. Defant gave a dynamical treatment of the spring tides of the Atlantic and Arctic regarded as one basin.† He applied to the combined basin of the Atlantic and Arctic the same methods which had been successful for such narrow seas as the Red Sea and the Adriatic. He considered two phases of a longitudinal oscillation. In each phase the transport of water is assumed to vanish at the northern end and the elevation to agree with observations at the southern end. All the generating forces are assumed to be allowed for in one of these phases, so that the other phase is a co-oscillation with the water of the Southern Ocean. Defant makes no allowance for the dissipation of energy. With one exception, the nodes of one phase are very near to the nodes of the other phase, so that their combination is practically a standing oscillation. There can be very little progression either northwards or southwards. When the rotation of the Earth is allowed for, transverse surface gradients are introduced, except quite near to the equator. In the North Atlantic the surface rises to the right of the current, while in the

* R. Sterneck, *Sitz. Akad. Wiss. Wien*, IIa, 129, 131, 1920.
† A. Defant, *Ann. Hydrog. Marit. Meteorol.*, 52, 153, 1924.

South Atlantic the surface rises to the left of the current, and of course in both cases it oscillates with the current. We thus have a series of amphidromic regions, the co-tidal lines going round in the anti-clockwise direction in the North Atlantic and Arctic, and in the clockwise direction in the South Atlantic. This means that a wave progresses northwards up the coast of South America and of Western Europe, and that a wave progresses southwards down the coast of North America and West Africa. There is, of course, no south-going wave down the coast of West Africa. But then Defant has not yet allowed for the transverse generating forces, and this difficult calculation he does not do adequately. His forced transverse oscillations are chosen so as to make the combination of all his oscillations agree with observations on the coasts. Fig. 5 shows Defant's co-tidal chart, but it may be subjected to some very serious criticisms:

1. He assumes that longitudinal generating forces produce only longitudinal currents, and that transverse generating forces produce only transverse currents. Owing to the Earth's rotation, these assumptions are not valid.
2. His total neglect of dissipation is very serious.
3. His forced transverse oscillations are without value.
4. The large range which he obtained on the arctic coast of Canada is not in agreement with observations.
5. The small range which he obtained at the Azores is not in agreement with observations.

In 1932, Defant abandoned his work of 1924 and confined his attention to the neighbourhood of the central line of the ocean and also to the part of the Atlantic south of Iceland.* He assumed:

1. That the rotation of the Earth does not affect tidal conditions on the central line.
2. That the transverse generating forces do not affect tidal conditions on the central line.
3. That there is no dissipation of energy to the south of Iceland.

His method does allow for the dissipation of energy outside the region which he considered, and in fact he stressed the importance of friction in the Arctic Sea. He considered two phases, differing by a quarter period, such that they agree with observations at the Azores and at Tristan da Cunha. This time the node of one phase coincides with the loop of the other, and their arrangement indicates that there is a northerly progression throughout the region considered. His results show good agreement with the observations at the islands. Defant's assumptions cannot be more than rough approximations, but it does seem as if conditions on the central line can be largely accounted for by means of longitudinal oscillations. Defant's longitudinal current-phases also increase from south to north. They appear to be in good agreement with the results of the "Meteor" expedition of 1925–27. But his longitudinal current-amplitudes are only about one-third of those observed by the "Meteor". I am not disposed to attach very much importance to this disagreement, because it is very difficult to obtain an adequate depth-mean of current when the observations are much affected by the presence of internal tides. But if we discredit the disagreement shown by the amplitudes we must not attach much importance to the agreement shown by the phases, because the accurate determination of current-phases from observations is subject to the same difficulty.

I now propose to describe an attack on the subject which I started some years ago, but which is still only in its early stages. As a preliminary, let us examine a series of precisely formulated mathematical problems. Consider a region of the Atlantic bounded by two parallels of latitude, namely those for 45° N. and 35° S. Suppose that we are given either:

* A. Defant, *Wiss. Ergeb. "Meteor"*, **7** (1), 273, 1932.

1. The general conditions to be satisfied along the coasts and the actual meridian-components of currents across the bounding parallels; or
2. The general conditions to be satisfied along the coasts and the actual tidal elevations on the bounding parallels; or
3. The general conditions to be satisfied at the coasts and the actual meridian-components of currents on one bounding parallel and the actual elevations on the other bounding parallel;

then the tidal elevations and currents are mathematically determinate all over the region considered, including their values on the coasts. For example, we might imagine the meridian-components of currents at 45° N. and 35° S. to be produced by suitably moving flexible pistons, and then in the first case it is clear that we have a definite physical problem. But we do not know either the actual currents or the actual elevations on the bounding parallels, whereas we do know the coastal elevations. Hence we consider the problem the other way round. Given the general conditions along the coasts and the actual values of the coastal elevations, the tides are mathematically determinate over the whole region considered, including the elevations and currents on the bounding parallels. This is my problem, but I propose to solve it by considering a series of other subsidiary problems of still different type.

Given the general conditions along the coasts and both the meridian-components of currents and the elevations along *one* bounding parallel, the tides are still mathematically determinate over the whole region considered.

This double condition along one bounding parallel has great advantages from a computational point of view. We can work steadily away from that parallel until we cover the whole region. Of course, in each subsidiary problem we have to take arbitrary distributions of current and elevation along the bounding parallel from which we start, but the object is to take such a series of distributions that a suitable linear combination of them will represent the actual distributions, whatever these may turn out to be.

I consider then a number of subsidiary oscillations. Each of these oscillations is a possible motion of the waters of the Atlantic under the conditions prescribed. These prescribed conditions include the general coastal conditions in all cases, and in each case they also include sufficient conditions along the parallel of latitude 35° S. to make the motion completely determinate.

I have not, as yet, introduced any dissipation of energy within the region considered, but this is quite consistent with any amount of dissipation of energy outside the region considered. My reasons for this neglect within the region are as follows:—

1. Jeffreys' total dissipation for the whole ocean did not include any contribution from parts of this region.
2. Defant's latest investigation did not involve any dissipation in this region.
3. It is desirable to keep the first attempt as simple as possible, and the introduction of coastal dissipation does complicate the details, though the general method allows for any amount of this.

I do not propose to describe the mathematical details by which these individual oscillations have been calculated. We proceed from one parallel of latitude to another 5° to the north of it, and much use has been made of an expansion-theorem, specially evolved for the purpose, which is being published by the London Mathematical Society.

It is possible that my treatment of the great irregularities of the basin formed by the Gold Coast and the north-east coast of Brazil may not yet be adequate. It is also possible that I have not yet taken sufficient terms in the series which represent the individual oscillations. I have still to examine these questions.

Fig. 6 shows the co-tidal and co-range lines of a forced oscillation. In this oscillation both the longitudinal and transverse generating forces are taken into account, and both the elevations and the meridian-components of currents are taken to be zero on latitude

35° S. The oscillation is thus independent of the tides to the south of latitude 35° S. The conditions satisfied along the southern bounding parallel secure that there is no passage of energy into or out of the Atlantic. But they put on a much bigger restriction than that, and keep the whole oscillation very small for some distance north of 35° S. This does not mean that only a small part of the tides of the Atlantic are generated within the Atlantic itself. If we put a barrier along any parallel of latitude, the cutting out

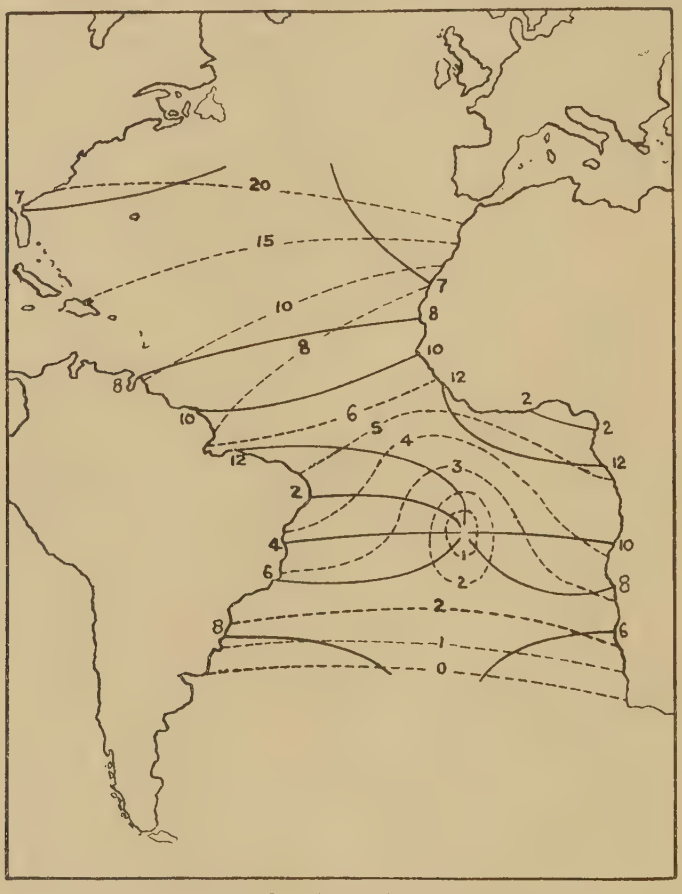

FIG. 6.—*Forced oscillation for M_2 constituent.*
Full lines give Greenwich lunar hour of high water.
Broken lines give amplitude in cm.

of the currents of this forced oscillation would require large multiples of the other oscillations.

The other subsidiary oscillations could be maintained by tides to the south of latitude 35° S. and are independent of the generating forces acting on the water of the Atlantic itself.

Fig. 7 shows an oscillation which I have called a north-going Kelvin-wave. It corresponds to a certain mean rate of northward progression of energy. As there are no generating forces and as there is no dissipation, this mean rate of progression of energy must be the same for all parallels of latitude. The particular type of motion assumed along the parallel 35° S. is that first investigated in 1879 by Sir W. Thomson for a canal on a flat Earth. The currents are directed along the meridians and both the elevations and the currents increase exponentially towards the west. No complication is produced by the curvature of the Earth along the parallels of latitude, but much

complication is produced by the curvature of the Earth along the meridians, by variations in the width of the ocean and by variations of depth. It is these complications which make the distribution shown in Fig. 7 vary from the simple conditions along the parallel of latitude 35° S. Continuous reflection along the coasts is allowed for, but if a complete barrier were put across the Atlantic, then of course the wave would arrive at an impossibility. We notice that there are large amplitudes in the south-west and in the north-east.

Fig. 8 shows the co-tidal and co-range lines of a south-going Kelvin-wave. This time there is a certain mean rate of southward progression of energy, which is the same for all parallels of latitude. Again, the currents in latitude 35° S. are along the meridian, but this time both the elevations and currents increase exponentially towards the east. This time it is not a question of following up the wave, but of finding what it must have been on any parallel of latitude in order to reach given values 5° to the south. We notice that there are large amplitudes in the north-west and the south-east.

Fig. 9 shows the co-tidal and co-range lines in what I have called a north-going Poincaré-wave. The type of motion along the parallel of 35° S. is next in simplicity to that of a Kelvin-wave; it was first studied by H. Poincaré in 1910 for the same canal as was considered by Sir W. Thomson. Instead of the transverse currents being zero, they now follow a sine-distribution from coast to coast. For a uniform flat canal there is a line of zero range parallel to the sides of the canal; the co-tidal lines run straight across the canal, but the time of high water changes by six lunar hours on crossing the line of zero range. These simple conditions hold in our case only along the parallel of latitude 35° S. On account of the curvature along the meridians and of the variations of width and depth, a line of zero range is too artificial and becomes replaced by a special form of amphidromic region. We notice that in the centre of the ocean the amplitudes remain small.

Fig. 10 shows the co-tidal and co-range lines of a south-going Poincaré-wave. This corresponds to a certain mean rate of southward progression of energy. The conditions in latitude 30° S. are similar to those for the north-going Poincaré-wave.

These Poincaré-waves are quite different from anything in Defant's work. They are the two simplest of an infinite series of oscillations, and it will probably be necessary to consider more. When in 1919 G. I. Taylor gave his determination of tidal motions in a rectangular gulf, he used both Kelvin-waves and the infinite series of Poincaré-waves.

It is now a question of assigning an amplitude and a time-origin to each Kelvin- and Poincaré-wave, so that when the four are added to the forced oscillation, the coastal elevations in the synthesis will agree with observations as far as is possible. As all the oscillations have been computed from prescribed conditions at the southern boundary of the region considered, it is desirable to make the agreement between synthesis and observation as good as possible in the southern part of the region, and then to discover where it begins to break down. As there are four free waves, I have made the sum of all the waves agree with observations at four points on the coasts, two on the east coast and two on the west coast. Two of these points I have chosen in latitude 32°·5 S. and the other two in latitude 7°·5 S., *i.e.* south of the Gulf of Guinea and Cape San Roque where the major irregularities of the basin begin.

I then find, as expected, that the north-going Kelvin-wave is the most important. I calculate that this wave must have an amplitude of 52 cm. and a high-water time of 1·8 hour at the south-west corner of the region considered, where it is greatest. The next most important wave is the south-going Poincaré-wave. This must have an amplitude of 35 cm. and a high-water time of 9·1 hours at the south-west corner, where it also is greatest. The other two free waves have each an amplitude of 22 cm. at the south-east corner, where they both are greatest in the south. The high-water time of the south-going Kelvin-wave at this point is 10·8 hours, and that of the north-going Poincaré-wave 0·5 hour.

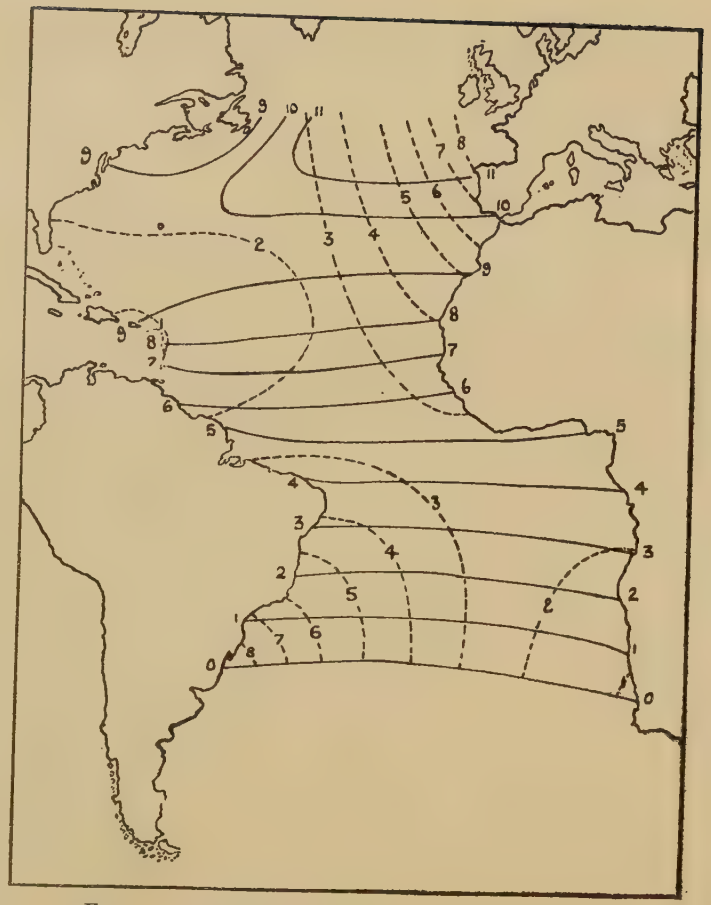

FIG. 7.—*North-going Kelvin-wave for M_2 constituent.*
Full lines give lunar hour of high water, origin arbitrary.
Broken lines give amplitude, scale arbitrary.

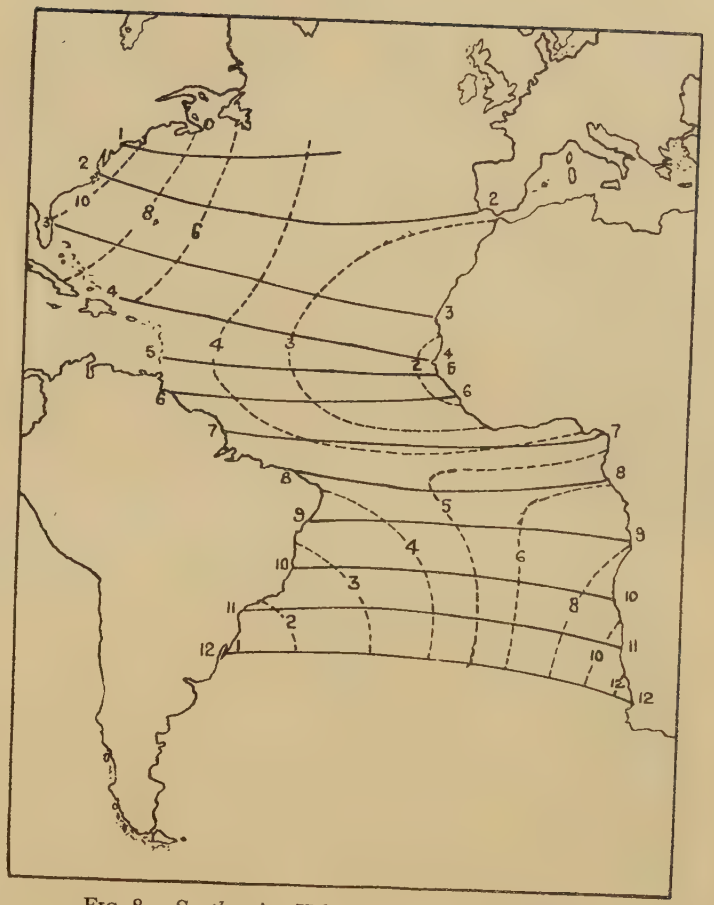

FIG. 8.—*South-going Kelvin-wave for M_2 constituent.*
Full lines give lunar hour of high water, origin arbitrary.
Broken lines give amplitude, scale arbitrary.

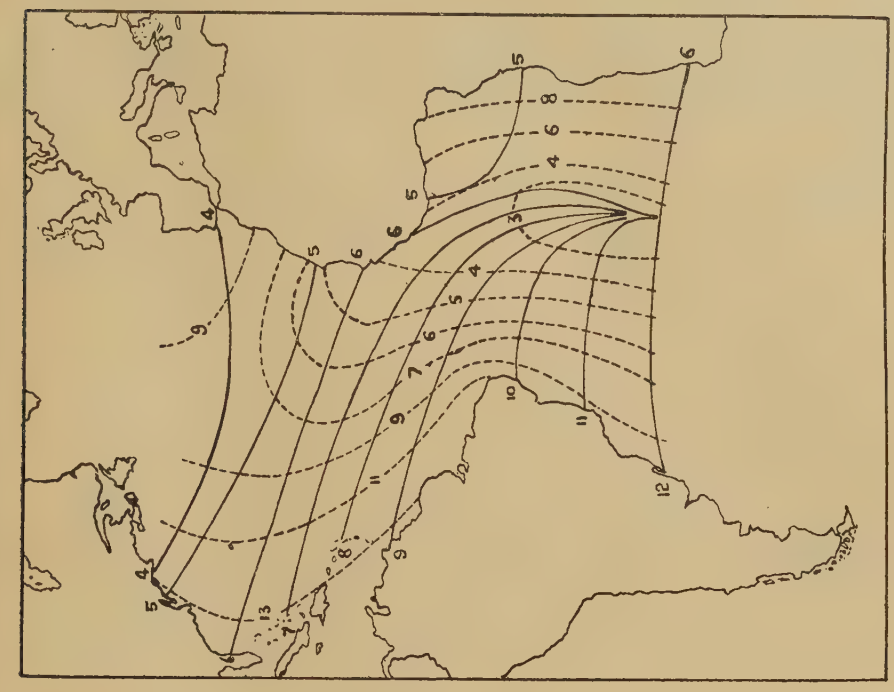

FIG. 10.—*South-going Poincaré-wave for M_2 constituent. Full lines give lunar hour of high water, origin arbitrary. Broken lines give amplitude, scale arbitrary.*

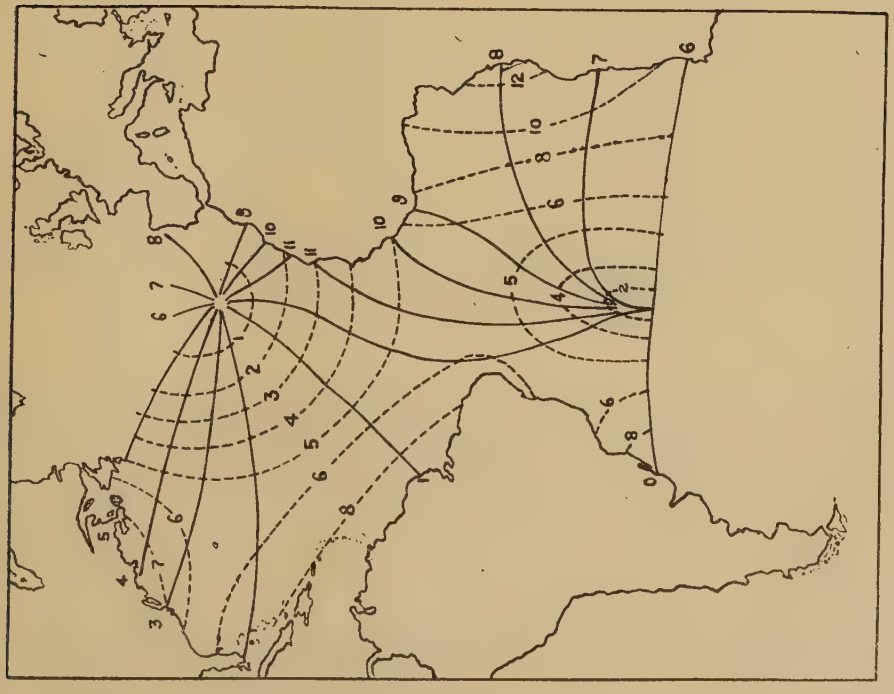

FIG. 9.—*North-going Poincaré-wave for M_2 constituent. Full lines give lunar hour of high water, origin arbitrary. Broken lines give amplitude, scale arbitrary.*

The shorter lines of Fig. 1 show the lunar times of high water and amplitudes in the synthesis. They show fairly good agreement with the results of observations to the south of the equator, but they diverge very considerably to the north of the equator. Part of this divergence may be due to the deficiencies of the individual waves, as already indicated, but it may also be due to my not having taken a sufficient number of Poincaré-

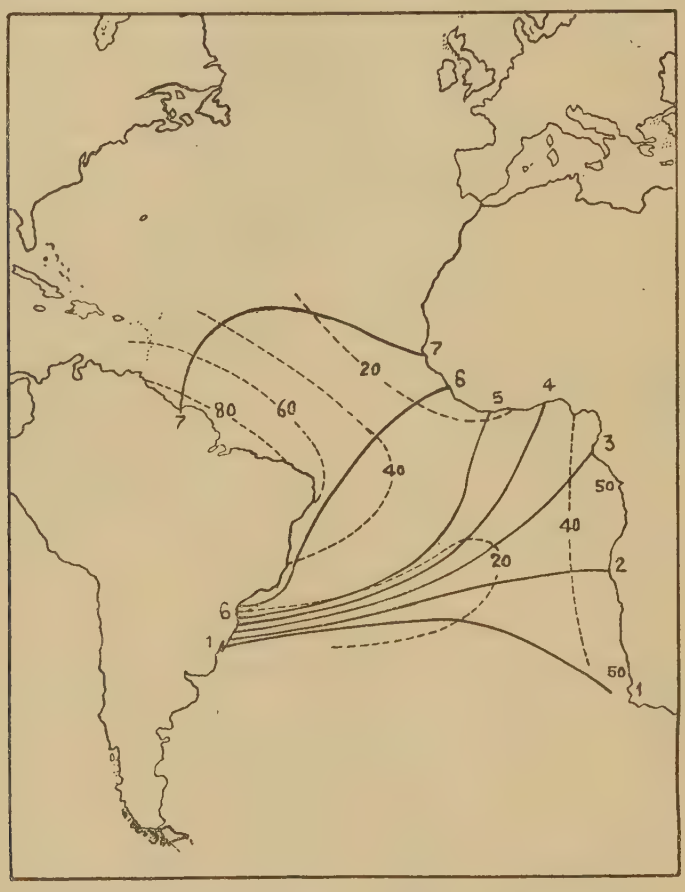

FIG. 11.—*Synthesis of subsidiary oscillations for M_2 constituent.*
Full lines give Greenwich lunar hour of high water.
Broken lines give amplitude in cm.

waves. The first two Poincaré-waves are somewhat special in that, for the particular dimensions of the Atlantic, at the southern boundary they progress along the meridians. All the others, at the southern boundary, progress along parallels of latitude.

Fig. 11 shows the distribution of co-tidal and co-range lines over the southern part of the region considered, according to the synthesis of waves at which I have arrived. I believe that this is the first time that co-range lines have been drawn right across the Atlantic. They show a trough of low amplitude reaching the coast of South America at about 32° S.; this trough contains a point of minimum amplitude in the middle of the South Atlantic.

By improving the determination of the individual subsidiary oscillations and by increasing the number of these oscillations, I hope to obtain a solution of the dynamical equations which will agree with the observations along greater lengths of coasts.

AN ANALYSIS OF BRITISH METEOR DATA: PART 2. ANALYSIS

J. G. Porter

(Received 1944 August 30)

Summary

The material is classified and grouped for analysis (§§ 1–3). The discussion of errors and their correlations leads to the important conclusion that errors of all types tend to increase with the length of path and the brightness of the meteor (§§ 4–7).

Correlations between the various physical quantities are tabulated (§ 8).

A comparison of data for the main showers and for all other meteors shows that, if the comparison is made in accordance with the rules of statistical analysis, there is no essential difference between the data for shower and non-shower meteors. In particular, the heights of comparable groups are significantly the same, and the so-called seasonal effect is really of a statistical nature (§§ 9–12).

A study of observed speeds leads to the definite conclusion that the low speeds as given by observers are real, and that meteors are retarded by the atmosphere before they become visible. A reasonable explanation of the anomalous appearance of telescopic meteors is thus afforded (§§ 13–17).

The complete absence of any direct evidence in support of the supposed excess of hyperbolic velocities among meteors is significant. Apart from a few exceptional cases, all meteors are members of the solar system, and behave as such (§§ 18–20).

Classification and Weighting of Data

1. The reduction of the multiple observations has been described in a previous paper * and the reduction of all the remaining material has now been completed, and a card-index formed of all the data so obtained. Each card contains the complete data for one meteor, entered in an unsymmetrical manner so that the eye can rapidly distinguish the required quantity. Most of the advantages of the punched-card system are obtained in this simple manner, and the subsequent sorting of the cards into groups is greatly facilitated, while the amount of purely clerical work is reduced to a minimum.

2. As a necessary preliminary, the cards were first sorted into two groups of shower and non-shower meteors. By arranging the cards primarily in chronological order, and secondarily in order of radiant, groups having similar radiants at the same time of year were obtained, the nature of the parabolic orbit being taken into account in cases of difficulty. These groups were subsequently identified, and are made up as follows:—

Perseids (July 22–Aug. 19)	152
Orionids (Oct. 16–29)	35
Taurids (Nov. 2–19)	28
Leonids (Nov. 15–17)	27
δ Aquarids (July 27–Aug. 18)	15
α Capricornids (July 23–Aug. 10)	14
Camelids (Aug. 6–11)	8
Quadrantids (Jan. 1–7)	8
Lyrids (Apr. 13–24)	6
β Piscids (Aug. 27–Sept. 18)	5
Total	298

The limiting dates for each of these groups is given above, and it will be seen that the limits are in some cases wider than those normally accepted. This is clearly due to the method of selection, which aimed at being independent of any previous knowledge of radiants. The Camelids in the list form a compact group with a centre about 72°, +58°; the remaining showers are well known.

* *M.N.*, 103, 134, 1943.

The remaining 480 meteors are of the type known as "sporadic". Without making any assumptions as to the meaning of this term, it may be taken that this group contains a small number of shower meteors consisting of groups (sub-centres) of fewer than 5 meteors; or, as in the case of the August meteors, of so diffuse a nature as to make the task of selecting them as shower or non-shower almost impossible. The total number of these shower-type meteors may amount to 10 per cent. of the total, but no more, and the sorting may be taken as sufficiently complete.

3. The R.M.S. errors for the duplicate observations are of the same order as those obtained from the multiples, hence we may have the same confidence in the results. The following table shows the errors for all meteors, and these results were used as a basis for weighting the measured quantities:—

	ΔH_1	ΔH_2	ΔM	ΔP	ΔS	ΔE_1	ΔE_2
	km.	km.		km.	km./sec.	°	°
Multiples	5·7	4·5	1·05	14·8	11·3	2·81	2·72
Duplicates	6·6	4·9	0·78	12·2	10·1	2·72	2·65
All meteors	6·5	4·9	0·80	12·4	10·2	2·73	2·65

In this table ΔS is the mean difference in speeds; this was found to be a better guide to the value of the estimates of speed than the original percentage ΔT.

Weights were assigned on a scale of 5, full weight being given to a quantity if its error were less than the corresponding figure in the above table. Quantities with an error more than three times the above errors were rejected. The weights assigned were in no case allowed to exceed the weight assigned to the radiant, for it is in just those cases where the angle of convergence of the apparent paths is small, that badly proportioned triangles are formed with the base-line, and the results are uncertain. The final weight assigned to the meteor as a whole was, in general, the lowest weight assigned to any of the measured quantities, so that the weighting never erred on the side of generosity.

Error Correlations

4. The main objective phenomena with which the observer is concerned are the apparent magnitude, apparent position and duration of flight. These quantities (or in the case of position, its derivatives mean angular path-length and apparent angular velocity) have been correlated with their errors, and the results are given in Table V. In view of the large frequencies (given in brackets) all of these values for the correlation coefficients must be considered significant.

TABLE V

Error Correlations

Error		Duration	Magnitude	Angular Path	Angular Velocity
Sliding	ΔE_1	+0·338 (688)	−0·175 (745)	+0·366 (558)	+0·102 (558)
Sliding	ΔE_2	+0·136 (690)	−0·083 (747)	+0·203 (558)	+0·152 (558)
Time	Δt	+0·573 (570)	−0·234 (570)	+0·446 (549)	−0·134 (548)
Magnitude	ΔM	+0·079 (669)	−0·338 (707)	+0·118 (545)	+0·066 (545)

Interpretation of these correlation coefficients is a matter requiring extreme care, for in the first place there is the common possibility that the correlation shown is really the sum of two more or less independent effects, and secondly there is frequently spurious correlation, especially where ratios, such as angular velocities, are concerned.

It is therefore proposed to deal only with those quantities for which the correlations appear to be most significant.

5. *The Sliding Errors.*—There is a clear positive correlation between the sliding errors and the apparent angular length of path. This is not due to any process of selection, as the only criterion of an accordance now adopted is the size of the sliding error.* Nor is it due to the effect of foreshortening, as the method of weighting has eliminated errors of this kind. There is thus no reason to doubt that the correlation is real. The explanation may be that a short meteor lies in one star field which may be embraced at a single glance, whereas the longer path puts a more difficult task on the observer's memory. There is also the point made by Öpik † that bright meteors (*i.e.*, in general, those with the longer paths) are often glimpsed outside the field of view, and in turning to record the meteor the observer has to focus his attention on, and identify, a new field, with consequent loss of precision in measurement. Whatever may be the cause of this correlation, however, there is no doubt that it exists, and the fact that the values of the computed quantities are, on the average, more reliable for the fainter, shorter paths, is one of extreme importance in the analysis which follows.

6. *The Magnitude Error.*—As previously shown by Öpik ‡, there is a definite positive correlation between the magnitude error and luminosity. This is not due to any excessively large errors affecting the very bright meteors, as the effect is clearly seen in meteors below magnitude −1. The correlation is no doubt due to the fact that the brighter the meteor, the further the observer has to go for standards of reference, and for meteors of planetary brightness or over, these must generally be recollected standards only. The use of comparison magnitudes after 1939 has not appreciably improved the R.M.S. errors in magnitude (0·65 mag. as compared with 0·70 mag. prior to 1939), but it has markedly reduced the number of large errors (9·6 per cent. being greater than 1·0 mag. as compared with 21·3 per cent. prior to 1939).

7. *Timing Errors.*—The quantity Δt is the difference in the proportionate times for the mean path, and involves errors in path length and duration, which cannot be separated. There is a strong positive correlation between timing errors and duration (+0·573) for which there seems no *a priori* reason, but the coefficient is too large to leave any doubt as to its reality, and supporting evidence is found in the value of +0·436 previously obtained § for the correlation of path errors and timing errors. Here again, the fact that the errors are generally smallest for the shorter and fainter paths is of importance in the analysis which follows.

Physical Correlations

8. A great deal of time has been spent on the correlations that exist between the various physical quantities derived from the meteors in this sample. The partial correlations so obtained were extremely confusing, however, owing to the large number of variables. The presence of many illusory correlations made the work almost impossible to interpret, and it was therefore decided to use the method of grouped means. The meteor-cards were arranged in order of, say, elongation, and divided into nine groups of equal weight, and the means for each group found.‖ Each group contains from 50 to 60 meteors—a number which it was hoped would prove sufficient to give approximately random distribution within the group of any quantity which is not definitely correlated with elongation. This hope was not fully realised, as will be apparent in what follows, and is shown very clearly in Fig. 2, but the principal correlations are nevertheless clearly established.

* The extent of the overlap of the two observed paths, which would naturally introduce an illusory correlation, is not now used as a criterion.

† *Publ. Obs. Tartu*, 25, 1, 1922.

‡ *Ibid.*, p. 21.

§ *M.N.*, 103, 144, 1943.

‖ The means are arithmetic, with the exception of S which is the R.M.S. value.

The other quantities in turn were then treated in the same way, and the results are shown in Tables VI and VII. The first column in each case refers to the quantity under discussion, and the means of each of its nine groups are given in the succeeding columns. The last column gives the theoretical parabolic velocity (geocentric) corresponding to the tabulated value of ϵ. Table VI contains the non-shower meteors only, and Table VII the Perseids, for which ϵ and V may be regarded as constant, and z is fixed within narrow limits. The smaller numbers of the Perseids made it necessary to reduce the number of groups in this case, but although the weight of each group is thus in theory less than that of the corresponding group of Table VI, the fact that ϵ and z are practically constant more than compensates for the loss of numbers. The Perseid groupings, in fact, are more likely to be a true guide to certain of the physical correlations than are the non-shower meteors. The presence of a definite correlation is indicated by an asterisk at the head of a column.

TABLE VI

Grouped Means (Sporadic)

(*a*) H_1 *Groups* (*Beginning Height*)

H_1	H_2	P	S	S/V	M	ϵ	T	z	V
	*		*	*		*			*
74·8	60·3	36·5	21·2	0·58	+1·08	95·7	1·69	52·9	29·2
85·8	75·5	35·2	18·1	·61	1·36	92·5	1·86	54·5	30·6
91·8	76·8	23·6	27·6	·71	1·34	84·0	1·07	43·3	34·9
96·7	82·5	23·4	23·7	·58	1·74	76·9	1·00	44·8	38·9
101·4	86·5	26·7	27·4	·61	1·13	74·2	1·26	48·7	40·5
106·5	92·2	27·8	36·3	·64	1·24	55·2	0·95	53·1	52·5
112·0	94·5	32·4	40·7	·64	0·49	41·9	0·89	52·7	60·3
118·8	97·0	39·6	45·5	·77	+1·04	46·3	1·05	52·4	57·8
141·3	107·8	61·3	60·8	0·99	−0·07	48·0	1·06	52·3	56·8

(*b*) H_2 *Groups* (*End Height*)

H_2	H_1	P	S	S/V	M	ϵ	T	z	V
	*					*	*		*
53·1	84·9	68·0	27·3	0·71	−0·86	95·5	3·22	48·2	29·3
71·1	87·9	36·8	28·4	·74	+0·80	89·9	1·69	51·5	31·8
77·6	93·0	25·2	27·0	·69	1·36	87·9	1·07	43·0	32·8
82·3	97·1	21·5	33·7	·67	1·43	79·4	0·99	42·4	37·5
86·3	98·3	24·0	26·7	·56	1·67	66·2	1·09	53·0	45·5
90·7	106·2	25·6	30·9	·63	1·43	64·8	1·05	44·3	46·3
95·1	109·8	27·2	36·3	·62	1·11	54·1	0·84	50·7	53·1
101·2	115·0	33·5	48·7	·72	1·15	42·6	0·72	58·9	59·9
115·1	131·4	41·9	48·6	0·74	+0·78	38·1	0·76	61·4	62·2

(*c*) P *Groups* (*Length of Path*)

P	H_1	H_2	S	S/V	M	ϵ	T	z	V
	*		*	*	*		*	*	
8·4	89·8	83·4	17·5	0·48	+2·82	89·5	0·58	36·9	32·1
12·5	97·1	87·9	28·0	·55	2·12	65·5	·63	39·9	45·9
15·8	100·5	89·7	25·7	·62	2·11	76·3	·79	44·9	39·2
18·8	98·4	87·1	28·6	·60	2·01	69·0	·87	49·2	43·7
22·9	100·2	87·2	31·6	·66	1·21	63·5	0·99	52·6	47·2
28·3	107·0	87·8	37·6	·67	0·35	57·6	1·10	44·7	50·9
36·1	108·8	89·7	41·8	·73	+0·65	59·6	1·50	58·0	49·7
52·1	106·0	83·5	51·8	·62	−0·79	67·8	1·65	58·2	44·4
107·7	116·9	81·2	50·2	0·98	−1·72	68·9	3·84	68·1	43·8

TABLE VI—*continued*

(*d*) *S Groups* (*Observed Speeds*)

S	H_1	H_2	*P*	*S/V*	*M*	ϵ	*T*	z	*V*
	*	*		*	*	*			*
10·2	85·7	76·4	16·7	0·39	+1·78	104·5	1·64	47·0	25·8
14·5	92·1	78·7	24·3	·49	1·36	92·9	1·68	46·7	30·4
17·5	98·5	80·4	36·5	·56	1·45	89·3	2·08	43·1	32·1
20·5	95·7	83·2	18·8	·56	1·90	75·4	0·92	43·5	39·8
24·0	100·6	84·8	28·3	·60	0·89	69·2	1·19	50·2	43·6
29·7	106·9	89·4	27·7	·70	1·24	61·5	0·94	46·0	48·5
36·1	107·1	89·7	46·0	·70	+0·25	48·3	1·28	53·7	56·6
45·0	112·2	94·7	36·8	0·86	−0·06	42·4	0·82	55·9	60·0
71·9	119·7	96·2	47·7	1·20	−0·30	39·6	0·69	54·2	61·5

(*e*) *S/V Groups* (*Ratio Observed/Theoretical Speeds*)

S/V	H_1	H_2	*P*	*S*	*M*	ϵ	*T*	z	*V*
	*		*	*	*				
0·29	89·8	79·8	18·1	14·3	+1·57	66·2	1·45	44·9	45·5
·40	99·1	88·5	21·8	20·0	1·48	63·6	1·27	53·4	47·1
·46	101·0	88·2	22·7	20·7	1·54	71·8	1·20	47·1	42·0
·53	104·6	91·4	23·1	27·9	1·34	55·7	0·89	48·6	52·2
·59	96·8	83·0	23·8	27·3	1·10	72·2	1·12	45·1	41·8
·66	102·1	87·0	29·2	34·9	1·25	61·5	1·01	52·5	48·5
·76	105·1	88·5	32·0	33·7	+0·88	75·0	1·27	46·4	40·0
0·92	110·1	83·5	59·7	48·2	−0·40	62·9	1·89	53·9	47·6
1·45	110·0	83·5	52·9	63·7	−0·18	75·3	1·15	48·3	39·8

(*f*) *M Groups* (*Zenithal Magnitude*)

M	H_1	H_2	*P*	*S*	*S/V*	ϵ	*T*	z	*V*
	*		*	*	*		*		
+3·89	94·7	86·9	14·1	24·3	0·53	74·5	0·76	47·4	40·3
2·96	102·3	90·2	19·3	29·4	·62	73·2	·81	42·8	41·1
2·34	99·2	88·9	21·9	29·4	·62	65·6	·85	49·4	45·8
1·78	99·1	84·5	24·8	32·2	·60	63·0	0·94	52·5	47·5
1·24	101·7	86·5	26·3	36·5	·66	63·6	1·00	46·0	47·1
0·81	109·4	86·4	39·0	39·8	·76	65·8	1·23	53·0	45·7
+0·06	103·0	85·6	37·2	39·0	·75	61·0	1·47	48·3	48·8
−1·36	110·0	88·5	42·8	38·7	·66	58·1	1·48	54·8	50·6
−4·22	110·0	77·9	80·1	45·1	0·92	64·1	3·02	61·2	46·8

(*g*) *Elongation Groups*

ϵ	H_1	H_2	*P*	*S*	*S/V*	*M*	*T*	z	*V*
	*	*		*			*		*
18·1	116·2	100·8	38·4	50·2	0·66	+1·00	0·79	61·7	70·3
30·3	113·6	98·2	34·0	50·2	·69	0·51	·79	57·2	66·0
41·5	112·5	92·5	30·1	44·5	·69	0·70	·71	52·9	60·5
55·7	105·3	89·9	31·5	31·9	·57	0·77	0·92	49·5	52·2
67·5	103·5	85·0	27·7	30·9	·58	1·49	1·18	45·3	44·6
78·3	100·3	80·0	39·4	30·9	·67	1·30	1·41	50·8	38·1
90·2	91·4	76·3	33·4	20·5	·60	1·27	1·58	49·0	31·7
104·6	95·2	78·8	23·7	22·4	·79	1·25	1·26	36·7	25·7
131·2	90·6	69·7	43·7	18·4	0·79	+0·72	2·51	54·3	19·6

TABLE VI—*continued*

(*h*) *T Groups* (*Duration*)

T	H_1	H_2	P	S	S/V	M	ϵ	z	V
		*	*	*		*	*		*
0·34	105·6	96·9	14·2	46·9	0·78	+2·36	47·6	48·5	57·0
·51	104·2	91·3	19·4	44·6	·78	1·54	54·8	43·9	52·7
·61	101·7	89·5	18·4	34·4	·66	1·53	64·1	41·9	46·8
·69	103·3	89·9	19·4	33·8	·66	1·34	74·4	42·4	40·4
·82	99·9	86·1	20·8	30·8	·54	1·36	65·3	42·7	46·0
0·98	99·5	83·3	27·3	33·6	·67	1·13	73·1	48·0	41·1
1·20	105·9	88·4	35·3	33·3	·61	0·76	62·4	54·2	47·9
1·73	101·4	82·9	41·7	29·9	·66	+0·11	81·9	57·7	36·1
4·30	97·0	65·5	85·0	21·6	0·68	−1·58	98·9	60·7	27·8

(*k*) *z Groups* (*Zenith Distance of Radiant*)

z	H_1	H_2	P	S	S/V	M	ϵ	T	V
			*			*		*	
17·1	97·6	78·5	16·4	26·0	0·66	+1·47	89·8	0·95	31·9
28·7	106·8	85·0	24·1	38·7	·76	1·04	69·8	·84	43·2
38·3	100·5	85·1	20·3	27·9	·58	1·79	72·7	·90	41·4
44·3	104·3	88·0	23·1	38·7	·68	0·81	64·7	·89	46·4
50·3	109·2	88·7	31·8	34·2	·60	1·04	57·9	0·98	50·8
56·7	98·8	83·5	28·0	33·2	·66	1·09	74·5	1·15	40·3
63·7	103·4	85·8	40·4	36·4	·71	1·06	68·6	1·67	43·9
72·3	104·2	89·6	48·3	41·3	·71	0·65	58·8	1·57	50·2
83·2	102·0	92·9	68·6	38·1	0·70	+0·25	60·5	2·74	49·1

TABLE VII

Grouped Means—Perseids

Mean $\epsilon = 39°{\cdot}2$ $V = 61{\cdot}7$ $z = 49°$

(*a*) H_1 *Groups* (*Beginning Height*)

H_1	H_2	P	S	S/V	M	T
	*	*			*	*
99·3	85·4	22·7	39·7	0·64	+1·00	0·59
108·3	93·1	24·2	40·0	·65	0·59	·63
112·4	97·7	24·0	39·7	·64	+0·52	·64
116·0	97·3	26·4	39·5	·64	−0·50	·68
127·7	101·3	40·6	55·9	0·91	−0·39	0·75

(*b*) H_2 *Groups* (*End Height*)

H_2	H_1	P	S	S/V	M	T
	*	*			*	*
80·3	104·6	36·6	48·3	0·78	−0·82	0·77
91·0	111·2	30·9	41·6	·67	0·83	·77
95·9	115·2	30·9	46·3	·75	−0·44	·71
100·0	111·6	18·4	36·3	·59	+1·66	·52
107·2	120·5	20·8	43·2	0·70	+1·74	0·51

TABLE VII—*continued*

(*c*) *P Groups* (*Length of Path*)

P	H_1	H_2	*S*	*S/V*	*M*	*T*
	*	*	*	*	*	*
12·5	106·3	98·2	34·3	0·56	+2·57	0·41
17·3	110·7	98·9	38·9	·63	1·78	·48
24·2	113·2	96·0	40·9	·66	+0·20	·62
33·0	112·1	91·1	44·0	·71	−1·24	·79
50·7	121·0	90·5	55·7	0·90	−2·00	0·98

(*d*) *S Groups* (*Observed Speeds*) *and S/V Groups*

S	H_1	H_2	*P*	*M*	*T*	*S/V*
	*		*	*		*
27·2	108·2	95·7	18·9	+1·01	0·69	0·44
34·0	109·6	96·2	22·4	0·86	·66	·55
39·8	111·5	96·4	21·5	+1·05	·54	·65
45·7	113·4	94·0	30·8	−0·61	·68	0·74
61·7	120·4	92·4	43·9	−0·97	0·73	1·00

(*f*) *M Groups* (*Zenithal Magnitude*)

M	H_1	H_2	*P*	*S*	*S/V*	*T*
	*		*	*	*	*
+3·44	108·5	97·6	16·2	41·0	0·67	0·43
1·79	112·3	98·9	19·5	39·6	·64	·50
+0·46	111·2	96·2	23·7	40·7	·66	·61
−1·07	115·3	94·9	33·1	42·2	·69	·84
−3·40	116·0	86·8	45·6	52·5	0·85	0·91

(*h*) *T Groups* (*Duration*)

T	H_1	H_2	*P*	*S*	*S/V*	*M*
		*	*			*
0·36	110·2	101·0	14·0	42·0	0·68	+2·59
·50	107·6	94·2	18·9	40·4	·66	1·66
·60	114·6	97·0	25·8	44·4	·72	+0·05
0·76	113·9	92·6	33·5	45·5	·74	−1·04
1·06	116·9	90·1	45·1	44·3	0·72	−1·91

9. It is not proposed at this point to make any detailed study of these correlations, but to deal only with the heights and speeds. Now the dominant factor among those which affect the heights is clearly the elongation of the radiant (or the equivalent velocity V), the smaller elongations giving the greater heights. This must be a velocity effect, the smaller elongations being of necessity associated with the higher velocities. Smaller effects due to magnitude and zenith distance, etc., are also present, but the effect of elongation is outstanding, and is seen also among the shower meteors. This is shown in Table VIII, which gives the means and standard errors for the principal

showers in this sample, arranged in order of elongation. The corresponding quantities for the sporadic meteors are given also for comparison.

TABLE VIII

Means and Standard Errors

	ϵ	H_1 (km.)	H_2 (km.)	P (km.)
	°			
Leonids	10·5	125·6 ±2·34	94·1 ±2·00	56·2 ±5·70
Orionids	24·1	119·7 ±2·14	100·9 ±1·79	27·0 ±2·23
Perseids	39·2	114·9 ±1·32	94·7 ±0·98	31·7 ±1·61
δ Aquarids	69·4	101·1 ±4·43	89·0 ±4·39	49·9 ±8·22
Taurids	81·4	101·6 ±2·24	77·7 ±1·97	33·5 ±3·49
α Capricornids	93·0	95·5 ±4·25	85·3 ±3·42	22·5 ±3·64
All showers	44·9	112·3 ±1·03	91·8 ±0·75	34·5 ±1·32
Sporadic	68·1	103·1 ±0·93	85·8 ±0·84	33·6 ±1·61

	S (km./sec.)	M	T (secs.)	S/V	No. $\downarrow$ s
Leonids	57·8	−1·78 ±0·45	1·23 ±0·20	0·76	27
Orionids	51·6	+1·20 ±0·30	0·65 ±0·06	0·69	35
Perseids	43·4	+0·51 ±0·19	0·66 ±0·02	0·68	152
δ Aquarids	29·3	+0·63 ±0·48	1·91 ±0·37	0·61	15
Taurids	25·7	−0·26 ±0·64	1·52 ±0·18	0·65	28
α Capricornids	24·2	+0·75 ±0·30	1·31 ±0·05	0·67	14
All showers	41·7	+0·39 ±0·14	0·93 ±0·04	0·67	298
Sporadic	34·9	+1·00 ±0·10	1·25 ±0·08	0·67	480

10. It is seen from Table VIII that the average shower meteor appears and disappears at greater heights than does the average "sporadic" meteor, a fact which has been previously reported * and is the subject of some speculation. But such a simple comparison of two classes is not in accordance with the principles of statistical analysis for it breaks all the rules of simple sampling. In a simple comparison of this kind, the two groups are drawn from very different ranges of elongation, while other varying factors such as magnitude and zenith distance will each have its effect on the height. Most of the shower meteors in any published list are, in fact, Perseids, and have a very restricted range in elongation and zenith distance (and probably in magnitude also)†, all of which are seen from Table VI to affect the heights. It is clearly illogical to compare the group of shower meteors, in which the sampling is necessarily stratified, with the remainder, in which there are no such restrictions.

11. If, however, we compare the shower meteors with sporadic meteors having a similar range in ϵ and in M, we shall be better able to make comparisons. It is found that there is no need to be very exacting about this, and in Table IX some of the principal showers are compared with the corresponding groups of sporadic meteors. In Group A, those having the same range of dates and of ϵ and M were used, while Group B has the same range in ϵ and M, but covers *all other dates* in the year. No particular care was taken over selecting the meteors—all those which came within the required ranges were used—and since the distributions of ϵ and M are different within these ranges, the

* Öpik, *H.A.*, **105**, 589, 1937.

† Because the observer normally records the brightest meteors when the rate is high.

means of these variables will still differ slightly. Yet it is hardly necessary to use the mathematical equipment of the statistician to show that the mean heights are now significantly the same. Had the ranges been restricted still more, so as to give the same means in ϵ and M, while using also similar restrictions in other measurements such as zenith distance, the mean heights would probably be even more closely identical. There would thus appear to be no doubt at all that the height of appearance and disappearance of a meteor is determined mainly by the values of ϵ and of M, and that in this respect both shower and sporadic meteors behave in an identical manner.

TABLE IX

Significance of the Means

		Shower	Sporadic	
			Group A (same dates)	Group B (all other dates)
Leonids	H_1	125·6 ±2·34	...	121·2 ±4·52
$\epsilon = 10°·5$	H_2	94·1 ±2·00	...	101·3 ±3·17
	n	27	...	21
Orionids	H_1	119·7 ±2·14	117·0 ±4·06	114·4 ±1·90
$\epsilon = 24°·1$	H_2	100·9 ±1·79	99·5 ±2·77	101·1 ±1·65
	n	35	14	53
Perseids	H_1	114·9 ±1·32	108·8 ±2·15	112·0 ±2·56
$\epsilon = 39°·2$	H_2	94·7 ±0·98	93·2 ±2·73	94·4 ±2·20
	n	152	26	29
δ Aquarids	H_1	101·1 ±4·43	101·9 ±2·99	100·3 ±2·23
$\epsilon = 69°·4$	H_2	89·0 ±4·39	84·9 ±1·96	83·8 ±2·49
	n	15	16	36
Taurids	H_1	101·6 ±2·24	90·9 ±4·43	98·8 ±2·94
$\epsilon = 81°·4$	H_2	77·7 ±1·97	78·8 ±3·49	77·9 ±1·94
	n	28	14	62
α Capricornids	H_1	95·5 ±4·25	100·9 ±3·24	93·3 ±2·09
$\epsilon = 93°·0$	H_2	85·3 ±3·42	82·7 ±2·24	80·5 ±2·48
	n	14	13	37

12. It is also seen from Table IX that the season of the year has no influence at all on the heights, the means for Group B being significantly the same as those for Group A. This is contrary to the so-called "seasonal effect" as announced by Öpik*, who has published figures showing the effect in the beginning, middle and end heights, and in the difference $H_1 - H_2$. But in actual fact, the effect is shown quite well by most of the other measured quantities—strongly by ϵ and by S, and less noticeably by M and T. In order to demonstrate this point, the sporadic meteors in this sample have been divided into convenient groups (unfortunately of widely varying weights) and the means for each group found. These are given in Table X, and the values of H_1, H_2, ϵ, S, M and $H_1 - H_2$ are plotted in Fig. 1, using the variate-difference method, in which the ordinates represent the difference between the value for the date given and that for the previous date. In such a sample, where the quantity under discussion is affected by

* *H.A.*, 105, 582, 1937.

the changes in several variables, any system of smoothing is bound to cause fictitious changes, and no smoothing has been employed here or elsewhere in this work.

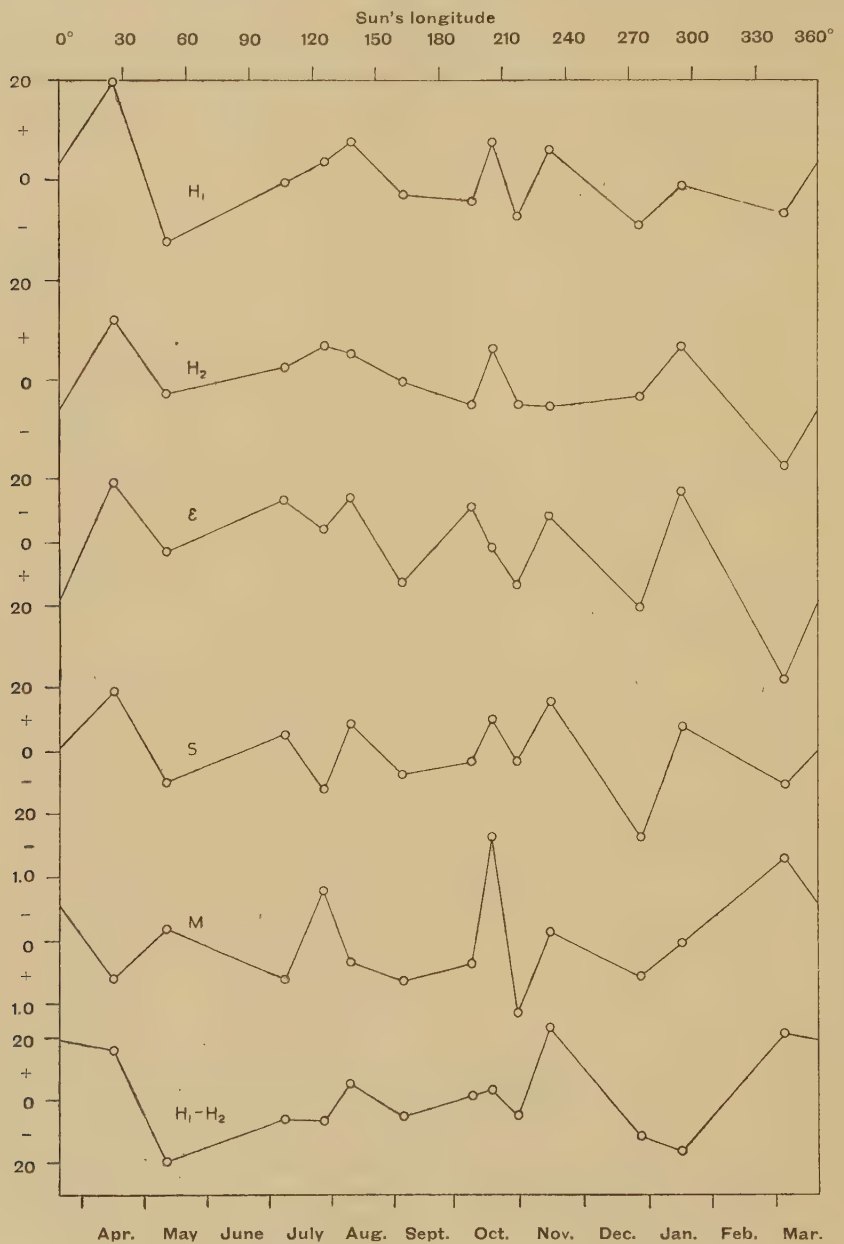

FIG. 1—*The "Seasonal" Variation.*

The similarity between the curves is obvious, although the influence of ϵ on the heights is clearly modified in some cases by the variations in M. The fluctuations of H_1 and H_2 are thus due to changes in the other variables, and it is obvious that the seasonal changes are entirely of a statistical nature. The dependence of the heights upon elongation is the dominant cause of the effect. The average elongation varies at

different seasons, and the accumulated effect of a large number of meteors with small elongations (as in August and October) raises the average height at these times. There is thus no seasonal variation in the height of meteors due to unknown causes. It is primarily a fortuitous variation in elongation, and if the mean elongation varies in this way, it is clear that there is no possibility of a *uniform* distribution of meteor directions about the Sun.

TABLE X

The Supposed "Seasonal Variation"

	☉	H_1	H_2	ϵ	S	M	No. ↓
	°						
1	275·0	98·3	77·9	83·1	25·7	+1·6	24
2	295·6	97·1	84·8	66·8	34·3	1·6	31
3	343·5	90·4	67·5	110·0	24·2	0·3	15
4	25·3	110·1	79·4	90·7	43·4	0·9	22
5	50·5	97·4	76·6	87·4	33·8	0·7	24
6	106·5	96·7	79·0	73·7	39·7	1·3	19
7	125·3	100·2	85·8	69·4	28·1	0·5	31
8	138·8	108·0	90·9	55·1	37·3	0·8	122
9	163·1	105·1	90·5	67·7	30·6	1·4	50
10	195·9	100·6	85·5	55·9	28·2	1·7	33
11	205·3	108·4	91·6	57·3	38·8	0·1	34
12	217·4	101·1	86·6	70·8	36·2	1·2	56
13	232·6	107·3	81·3	62·4	52·4	+1·0	19

The Observed Speeds

13. The observed speeds of meteors have been consistently ignored by most investigators on the tacit assumption that they are always greatly in error. It is an assumption that should be tested, however, and for this purpose it may be confidently asserted that the speeds in the present sample are at least as worthy of credence as those of the Arizona Meteor Expedition. For although the Arizona durations may have been more accurately determined, the paths were so erroneous that it was necessary to use average values based on a number of assumptions. Olivier * has strongly criticised the results of this expedition on various grounds, in particular that they do not include any data for the major showers, and that there is therefore no check upon the results by a simple comparison of shower and non-shower meteors. Such a check is obviously of fundamental importance, but it must be used not only to compare the speeds of the two groups, but also to show that the speeds are mutually consistent. In the present series of observations, the close correlation between the observed speed S and the parabolic velocity V may be seen for the various values of ϵ in Table VI (*g*). The same close correlation is also seen for the shower meteors in Table VIII, in which the observed speeds of the principal showers are separately tabulated. The effect of elongation on observed speed is shown graphically in Fig. 2, in which the continuous line represents the theoretical (geocentric) parabolic velocity V. The observed speeds for the showers, and for each of the nine groups in Table VI (*g*) are plotted, and are seen to lie close to a line which has its ordinates about 75 per cent. of those of the curve of parabolic velocity. The graph shows clearly the consistent behaviour of these speed-groups, and again establishes the fact that there is no significant difference between the shower and non-shower meteors. The close similarity between the two groups thus indicates that the sporadic meteors have speeds of the same order as the shower meteors.

14. The fact that the observed speeds of meteors are only some 75 per cent. (on the

* *P.A.*, 46, 325, 1938.

average) of the theoretical parabolic velocity cannot be dismissed, as has been done in the past, as due to errors of observation. For the results of photographic determination of speeds by different workers using different apparatus and methods also gives results of the same order. Thus in the work of Whipple *, of Millman and Hoffleit †, and in the recently published results of Elkin's early work ‡, the range of measured speeds is very similar to that of comparable bright meteors in the present sample. Nor is it possible to dismiss this defect of speed without explaining the fact that the distribution of speeds is decidedly skew, and at the same time accounting for the correlations that may be seen to exist between the observed speeds and the other measured quantities.

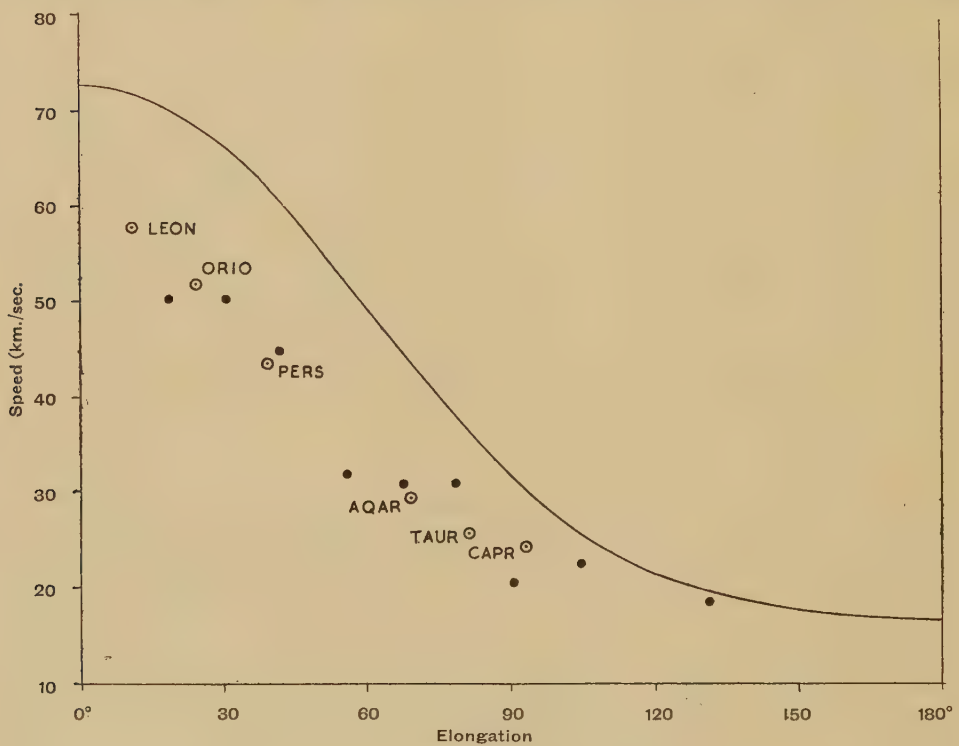

FIG. 2.—*Relation between Elongation and Speed.*

If in these different meteor groups the value of S/V is used (in order to avoid the more obvious effects of difference in original velocity) then it is seen from Tables VI and VII that whereas S/V is correlated with H_1, P and M, there is no correlation with H_2, V or T. On the other hand, the original speed, as measured by V, is correlated with both H_1 and H_2, and with T, but not with P. It is this curious difference in the correlations that is referred to above. The skew distribution of S/V is shown in Table XI in detail, and is apparent in all the columns, of whatever weight.

15. Now no explanation based on errors of observation can explain all of these facts. Random errors, for example, could not produce a skew distribution of this kind, nor would they give rise to the correlations enumerated above. It has been shown that the random errors are least with the short, faint meteors. The reduction in speed, however, is seen to be greatest with this class, so that the effect, if not real, must be due to systematic errors affecting all observers alike. Of the possible sources of systematic error, we may briefly discuss the following: (*a*) the use of harmonic mean

* *Proc. Amer. Phil. Soc.*, 79, 499, 1938 = *H.R.* 152.
† *H.A.*, 105, 601, 1937. ‡ Olivier, *A.J.*, 46, 41, 1937.

TABLE XI

Distribution of S/V

	Sporadic							Shower						
Weight	5	4	3	2	1	Σn	Σp	5	4	3	2	1	Σn	Σp
S/V														
0·0 –		1			1	2	5							
0·2 –	8	5	6	14	29	62	135	1	2	6	2	14	25	49
0·4 –	12	10	21	37	65	145	302	12	7	15	12	31	77	188
0·6 –	10	10	9	15	51	95	198	15	10	13	12	35	85	257
0·8 –	4	2	4	10	32	52	92	10	2	6	8	10	36	102
1·0 –	3	1	3	2	14	23	46	1	1	1	1	16	20	30
1·2 –	1	1	1		15	18	27				2	4	6	8
1·4 –				1	11	12	13	1				3	4	8
1·6 –					7	7	7					4	4	4
1·8 –		1			1	2	5					1	1	1
2·0 –			1	1	5	7	10					2	2	2
3·0 –					1	1	1							
			Median		0·59					Median		0·63		

speeds instead of arithmetic means, (*b*) the possible use by the observers of an erroneous time-scale, giving durations that are too great, (*c*) the possibility that observers consistently under-estimate their path-lengths, (*d*) the possibility of a time-lag between the moment when the meteor is perceived and the moment when the observer begins to record its position. Dealing with these in turn:

(*a*) The use of harmonic mean speeds cannot be the cause, as the defect in speed is shown equally in the groups of full weight 5, in which the A.M. and H.M. are identical.

(*b*) An error in time-scale is inadmissible because it fails to account for the skew distribution. The effect of such an error would be to give a normal distribution of values about a new mean; it could not possibly give rise to the correlations mentioned.

(*c*) The same criticism applies to a systematic reduction of path-length.

(*d*) The presence of a time-lag between perception of the meteor and the observation of its position may be regarded either as an over-estimate of duration for the observed path, or as a defect in path-length for the estimated duration. A *constant* error of this kind would account for the defect in speed, and also for the skew distribution; but it would also clearly cause a positive correlation between speed and duration, the slowest speeds being associated with meteors of short duration. This is not in accordance with the facts, for it may be seen from Table VI (*e*) (*h*) that there is no correlation between the duration T and the ratio S/V. The absence of this correlation is even more significant in the case of the Perseids (Table VII) where V is a constant. In this case the correlation of S with T gives $r = +0{\cdot}146$ ($n = 138$), a figure which is less than twice the standard error, and might well arise as a result of sampling fluctuations. It can only be concluded that there is no evidence for the presence of a time-lag among the observations, a conclusion which is confirmed by the experimental work of Boyd * on the timing of artificial meteors, in which there was no evidence of systematic errors of this or any other kind.

* *P.A.*, 44, 39, 1936.

We find, therefore, as was to be expected from the close agreement between this series and the photographic observations, that the defect in speed is not due to systematic errors.

16. It thus appears that the only possible explanation of this defect in speed lies in the retardation of the meteor in the Earth's atmosphere during that part of its flight *before* it becomes visible. This conclusion has been previously announced by Millman and Hoffleit from a study of meteor photographs:—

> "It is probable that many meteors are subject to considerable deceleration before they reach a luminosity high enough for photographic records. The velocity of shower meteors . . . shows that the Leonid and Perseid meteors had suffered a decrease in velocity of at least 10 km./sec., and the Orionid meteors probably even more".*

If this view be accepted, the curious correlations of S with the other physical quantities are seen to be the inevitable results of a retarding force. Thus the retardation is greater for small meteors (correlation with M and with P) and for meteors which have penetrated more deeply into the atmosphere (correlation with H_1). The fact that S/V appears to be independent of V would indicate that the actual numerical reduction of speed is greatest for the fastest meteors. These are the effects to be expected if the retarding force is of the type $F=\rho AV^2$, where ρ is the density of the atmosphere, and A is the area of the meteor. This type of retardation is assumed in many of the theories of meteors, which, however, do not envisage such a retardation *before* the meteor becomes luminous.

17. If, however, such a retardation exists, then the skew distribution of Table XI is immediately explained as due to the irregular distribution of H_1, P and M with which the ratio S/V is correlated. These correlations in their turn must be due to the influence of the size of the meteor and the density of the atmosphere. Moreover, the distribution of these correlated quantities is different in the two groups of shower and non-shower meteors, and this accounts for the fact that the distribution of S/V has a different mode in each group. Furthermore, the existence of such a retardation affords a simple explanation of the anomalous behaviour of telescopic meteors. It is a well-known paradox that meteors seen through the telescope appear to move no faster through the field than those seen with the naked eye, whereas owing to the magnification their angular velocity should be very much greater. Two possible explanations have been offered, one that these meteors are a special class becoming luminous at very great heights, and the other that they are all highly fore-shortened near their radiants. The first of these explanations is not now accepted, and the second is improbable. But if the original speed of a very faint meteor has been largely destroyed by atmospheric retardation, the telescopic appearance of these meteors is immediately explained. It is seen from Table VI (*f*) that the very faint meteors have very short paths and slow speeds. Extrapolating from this table, we may estimate that an eighth magnitude meteor would have a path-length of one or two kilometres, and a speed of 10 km./sec., or less, and at an average height of 80 km. at a distance of 150 km. from the observer would present an apparent path of rather less than one degree which it would cover in about 0·2 sec. The figures are very rough, but they show that the explanation is adequate.

The Supposed Hyperbolic Velocities

18. Throughout this paper the identical behaviour of the two groups of meteors has been apparent, and nowhere is there any evidence of the preponderance of hyperbolic velocities as claimed by Hoffmeister, Öpik and others. In fact, only 16 per cent. of the sporadic meteors in this sample have a value of S/V greater than unity, and most of these are of the lowest weight (Table XI). Moreover, even this small percentage cannot

* *H.A.*, 105, 612, 1937.

be taken as a proof of the presence of hyperbolic velocities, for 14 per cent. of the shower meteors have also S/V greater than unity, so that these figures merely represent the tail of the distribution curve. These figures, of course, do not take into account the effect of atmospheric retardation, but the exact similarity of behaviour of the shower and non-shower meteors shows that the correction for retardation could not greatly increase the percentage of true hyperbolic velocities. There is therefore nothing in the present sample (the accuracy of which has been demonstrated) to justify the belief in more than an occasional meteor with hyperbolic velocity.

19. As this is directly contrary to the hyperbolic theory which has been so widely accepted, it seems necessary to call attention to the very slender foundations on which that theory rests. The most important observational work cited in support of the hyperbolic theory is the following:—

*The von Niessl-Hoffmeister Fireball Catalogue.**—This contains the real paths of 611 fireballs, not one of which is a shower meteor, and of the total, 79 per cent. have hyperbolic velocities. Fisher † has strongly criticised this catalogue in a reasoned argument based on a comparison of shower and non-shower meteors. As such a comparison is a fundamental test, Fisher's criticisms stand as firmly to-day as when they were first written. It may also be noted that 44 per cent. of the paths in this catalogue were taken direct from the lists of Denning or Herschel, and, as has been shown ‡, very little reliance can be placed on their results. Many of the observations in this catalogue were the work of casual observers, and carry no weight at all.

Hoffmeister's Analysis of Denning's Results.§—This consists of a simple comparison of shower and non-shower meteors, from which Hoffmeister derives a mean velocity slightly greater than parabolic. The heterogeneous nature of Denning's results has been shown ‡, and as the work is a comparison of two differently stratified samples, the result has no validity.

Hoffmeister's Work on Hourly Rates.‖—The attractiveness of this indirect method of deriving the average velocity of meteors from the variation in their hourly numbers, and the skill with which the method has been worked out, all tend to obscure the simple faet that it *is* an indirect method resting on at least one very important assumption—that meteor radiants are distributed uniformly over the celestial sphere. That this cannot be the case is demonstrated in § 12 above, and the results of this indirect method are, in fact, entirely at variance with those of direct measurement.

The Work of the Arizona Meteor Expedition.—The results of this expedition are quoted by Hoffmeister ¶ and other authors of textbooks as an example of the direct measurement of meteor velocities. This is most decidedly not the case. Öpik's paths were so erroneous that his results had to be based on mere assumptions as to mean centres of radiation and mean heights. Nothing could be more remote from the direct determination of the heights, paths and speeds studied in the present paper. The Arizona results are open to other criticisms, which Olivier has made **, notably the curious absence of shower meteors, and the extraordinary positions of some of the centres of radiation. The absence of shower meteors prevents the fundamental check on reliability by a comparison of shower and non-shower meteors.

20. The methods by which hyperbolic velocities of the order 2·4 times the Earth's mean speed have been obtained are essentially indirect methods, each depending on at least one assumption. The present analysis shows that these assumptions are not justified, and leads to the following conclusions:—

(*a*) All meteors, whether belonging to the major showers or not, behave in the same way.

(*b*) Hoffmeister's assumption of a uniform distribution of meteor paths in space cannot be justified; the non-uniform distribution of meteor directions is responsible for the apparent seasonal variation of heights.

* *Denk. Akad. Wiss. Wien, math. nat. Kl.*, **100**, 1925.
† *H.C.*, 331, 1928; 375, 1932.
‡ *M.N.*, **103**, 137, 1943.
§ *A.N.*, **221**, 354, 1924.
‖ *Ast. Abh.*, **4**, No. 5, 1922; *Ver. Berlin-Babelsberg*, **9**, No. 1, 1931.
¶ *Die Meteore*, 30, 1937.
** *P.A.*, **46**, 325, 1938.

(*c*) There is no direct evidence for the existence of an excess of hyperbolic velocities. With a few doubtful exceptions, all meteors are members of the solar system.

These facts are based on the results of a standard statistical analysis of directly observed quantities, whose accuracy is beyond dispute. To reject such evidence in favour of results based on assumptions and empirical formulæ is the very negation of scientific method. The hyperbolic theory has hindered the progress of meteoric astronomy for many years, and in the opinion of the writer should be abandoned.

Grateful acknowledgment is made to Mr. J. P. M. Prentice, whose encouragement and criticism have been of the utmost value in a long and arduous task.

ON THE MECHANISM OF ACCRETION BY STARS

H. Bondi and F. Hoyle

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Summary

The mechanism of accretion is investigated in detail for the case in which the interstellar material contains a sufficient proportion of molecules to ensure that the temperature is everywhere small. It is found that there is no unique steady-state solution and that the situation at any given time depends on the perturbations suffered by the system. The gravitational interaction between the interstellar material and the stars leads not only to an increase in the masses of the stars but also to a decrease in their peculiar velocities. The importance of these effects to stellar dynamics and to the evolution of the stars is briefly discussed.

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1. *Introduction.*—The importance to stellar astronomy of a large rate of accretion of hydrogen by stars is well known. On the basis of certain physical and dynamical assumptions a simple argument was given by Hoyle and Lyttleton * for a rate of accretion according to the formula

$$\frac{4\pi\gamma^2 M^2 \rho_\infty}{V^3}, \tag{1}$$

where M is the mass of the star, γ is the constant of gravitation, V is the velocity of the cloud relative to the star, and ρ_∞ is the density of the cloud at very large distance from the star. The physical assumptions underlying this calculation were later shown to be satisfied † provided an appreciable proportion of the cloud is in molecular form, for in this case the temperature of the cloud remains small throughout the accretion process on account of the radiation of energy by the molecules. The existence of interstellar molecules throughout large tracts of the galaxy has now been confirmed by observation by McKellar and also by Adams.‡ Moreover, McKellar's observations provided direct evidence that the temperature of the interstellar material is far lower than is necessary to justify the assumptions of Hoyle and Lyttleton.† Thus there is important observational confirmation that the accretion problem can be simplified by regarding forces due to gas pressure as being in general small compared with gravitational forces. The object of the present paper is to obtain a rigorous solution of the accretion problem in this simplified form.

In section 2 an outline of the mechanism of accretion is given, and in section 3 the equations governing the steady state of accretion are formulated and discussed. It is found that the steady state equations together with the boundary conditions do not determine a unique solution to the problem, since it can be shown that a group of solutions can be found all giving an accretion rate greater than half the value given by (1). This remarkable property suggests that the situation at any given time must depend on the perturbations suffered by the system, and in section 4 a simple application of perturbation theory shows that perturbations will prevent the rate of accretion dM/dt exceeding the value given by (1). It is concluded, therefore, that the accretion rate at any given time

* *Proc. Cam. Phil. Soc.*, **35**, 405, 1939.
† *Proc. Cam. Phil. Soc.*, **36**, 424, 1940.
‡ *P.A.S.P.*, **52**, 187, 1940; *H.A.C.*, 526, 1940.

must satisfy the inequality

$$\frac{2\pi\gamma^2M^2\rho_\infty}{V^3} < \frac{dM}{dt} < \frac{4\pi\gamma^2M^2\rho_\infty}{V^3},$$

the exact value depending upon the nature of previous perturbations. In section 5 the extreme case of perturbation is considered in which the star initially in empty space enters a cloud of uniform density possessing a plane boundary. The setting up of a steady state in this case is considered, and it is shown that the accretion rate will tend to a value close to

$$\frac{dM}{dt} = \frac{2{\cdot}5\pi\gamma^2M^2\rho_\infty}{V^3}. \tag{2}$$

It is plausible to suppose that (2) gives a lower limit to the accretion rate since it refers to a steady state set up after a particularly violent perturbation.

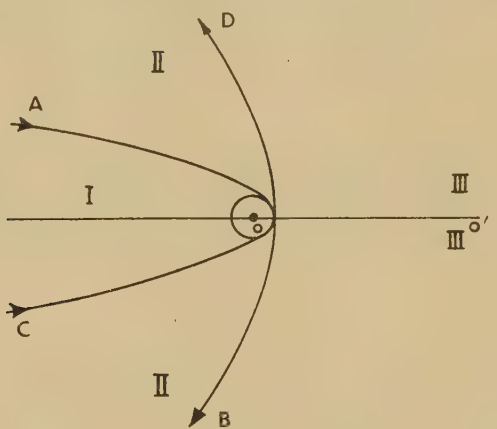

FIG. 1.

In section 6 the rate of change of V due to the drag of the cloud on the star is calculated. The value of dV/dt is found to be so large that the drag due to the cosmical cloud would appear to be of great importance in stellar dynamics, since it provides a mechanism whereby the interstellar material can control the peculiar motions of the stars. Such a mechanism introduces a stabilising influence into galactic dynamics that would appear to be of great significance. Moreover, the decrease of V with time reduces the time-interval required for the mass of a star to be increased by an appreciable factor. It is shown that the mass of a star may be significantly increased in about 1/10th of the period given in previous work.* This result implies that either the time-scale of the evolution of the stars is shortened by a factor of about 10, or that a smaller density of the cosmical cloud is sufficient to maintain the rate of evolution than was previously proposed.

2. In earlier work it was shown that provided the density ρ_∞ is sufficiently large for the mean free path of the particles to be small compared with $\gamma M/V^2$, then the process must be treated as a gas problem. The importance of the collisions between the particles can be seen by comparing the hypothetical case in which the collision cross-section is assumed to be negligibly small with the actual situation where the mean free path is small compared with $\gamma M/V^2$. The former case is illustrated in Fig. 1, in which the trajectories AB and CD are intended to represent the paths of particles just avoiding collision with the surface of the star. Then the paths AB and CD together with all similar trajectories divide space into three regions marked as I, II and III in Fig. 1. All particles moving

* *Proc. Cam. Phil. Soc.*, **36**, 325, 1940.

in region I strike the surface of the star. Region II is a single-stream region since there is only one trajectory passing through any point in the region, whilst region III is a multi-stream region since there are two trajectories passing through any point not on the accretion axis oo′ and there is an infinity of paths passing through any point on the axis.

If now the collision cross-section of the particles is restored to the gas-kinetic value, then owing to the fact that the temperature of the gas is small there will be no change in the properties of regions I and II since there is only one trajectory through every point in these regions. On the other hand, in region III particles will collide even though the temperature is small, and these collisions will tend to prevent the occurrence of two streams of material passing through each point not on the axis. Indeed it is clear that the multi-stream region cannot possess a dimension of much more than the mean free path in a direction measured along the trajectories. Now owing to the high density of material near the axis oo′, the mean free path * will then be much shorter than in regions where the density is comparable with ρ_∞. It follows, therefore, that the extension of the multi-stream region in a direction perpendicular to the axis must be very small compared with $\gamma M/V^2$. The multi-stream region must exist, however, since the stream-lines of the material cannot be diverted by gas pressure. Accordingly the mechanism of the

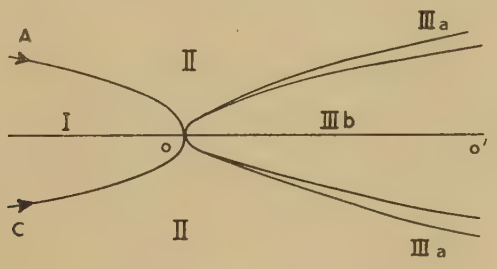

FIG. 2.

accretion process is determined by the four regions I, II, III*a* and III*b* shown in Fig. 2, where the region III*a* is a double-stream region with a thickness of the order of the mean free path and I, II and III*b* are all single-stream regions. The thickness of III*a* is clearly determined by the value of ρ_∞, and for large ρ_∞ this region is effectively a surface of discontinuity. In region III*b* although the temperature is low the pressure is nevertheless high on account of the very high density in this region. This pressure supplies an outward force on region III*a* that must balance the component of momentum transverse to oo′ of material entering III*a* from II. Indeed the transverse dimension of III*b* must adjust itself to make these two forces equal and opposite. The transverse dimension of III*b* accordingly depends on the temperature of the material in III*b* which, although small, is important in determining the size of the region.

3. If the temperature of the material is neglected and the collision cross-section of the particles of the material supposed to be negligibly small, then it is easy to show that the mass of material crossing the axis per unit length per unit time is

$$A = 2\pi\rho_\infty \frac{\gamma M}{V}, \tag{3}$$

whilst at the time the material crosses the axis the component of velocity parallel to the axis is V. Now since the transverse dimension of the region III*b* is small compared with $\gamma M/V^2$ (this must be the case on account of the low temperature due to radiation of energy by the molecules in the material), it is permissible to suppose that when gas-kinetic

* It can be shown that for distances from the axis small compared with $\gamma M/V^2$ the mean free path is proportional to the distance from the axis provided the temperature of the material can be neglected.

collisions take place the amount of material entering the region III*b* is still A per unit length per unit time and that the velocity of this material parallel to the axis is still V. Thus since A and V are independent of r, the only condition impressed on region III*b* that varies with r is the gravitational attraction of the star. The following equations can be set up if we assume that the pressure gradient parallel to the axis can be neglected compared with gravitational forces, and that the velocity v of material in III*b* is uniform over any cross-section and directed parallel to the axis.

$$\frac{d}{dr}(mv) = A \qquad \text{(conservation of mass)}, \tag{4}$$

$$\frac{d}{dr}(mv^2) = AV - \frac{Mm\gamma}{r^2} \qquad \text{(conservation of component of momentum in direction parallel to axis oo')}, \tag{5}$$

where mdr is the mass of material in III*b* lying between r and $r+dr$.

The step of neglecting the longitudinal component of the gas-pressure gradient can be justified by noting that equation (4) integrates to give

$$mv = A(r - r_0), \tag{6}$$

where r_0 is a constant of integration that is later shown to be of order $\gamma M/V^2$. Equation (6) shows that the material flows inwards towards the star for $r < r_0$ and outwards to infinity for $r > r_0$. Now since V is the parabolic velocity corresponding to a distance $r = 2\gamma M/V^2$, it follows that the time taken for material to move inward to the star from the neighbourhood of r_0 must be of order $\gamma M/V^3$, so that the value of m at $r = 0{\cdot}5\, r_0$, say, must be of order $A\gamma M/V^3$. The pressure P in III*b* is to good approximation given by the equation

$$P = \frac{A}{2\pi s}\sqrt{\frac{2\gamma M}{r}}, \tag{7}$$

where s is the radius of the cross-section of III*b* at distance r. The equation (7) gives the first-order approximation to the condition that the region III*a* remains in a fixed position owing to the outward pressure from region III*b* being balanced by the momentum of particles with transverse velocity $\sqrt{2\gamma M/r}$ entering III*a* from II. To compare the longitudinal gas-pressure gradient with the gravitational force of the star, consider a region bounded by the cross-sections of III*b* at distances r and $r+dr$, and by the inner surface of III*a*. Then the force acting on the material in this region due to the longitudinal pressure gradient is

$$d(\pi s^2 P) = A\sqrt{\gamma M/2}\; d(s/\sqrt{r}),$$

which is of order $A\sqrt{\gamma M/2}\,.sdr/r^{3/2}$ since ds/s is of the same order as dr/r, whilst the gravitational force is $m\gamma M dr/r^2$ and is of order $A\gamma^2 M^2 dr/r^2 V^3$. Thus since r is of order $\gamma M/V^2$ and s/r is small compared with unity, the ratio of the force due to gas-pressure gradient to the gravitational force is seen to be $<< 1$.

The second assumption used in deriving (4) and (5) is that v is constant over any cross-section of III*b* and directed parallel to the axis. This is seen to be the case since the ratio $s/r << 1$, so that a velocity of circulation of material transverse to the axis and small compared with V will produce this uniformity in a time $\gamma M/V^3$. The equations (4) and (5) are more conveniently treated by introducing the non-dimensional variables defined by

$$r = \frac{\gamma M}{V^2}x, \qquad v = Vy, \qquad m = Az\frac{\gamma M}{V^3} = z\,.2\pi\rho_\infty\left(\frac{\gamma M}{V^2}\right)^2.$$

Then we have

$$\left.\begin{aligned} y\frac{dy}{dx}+\frac{1}{x^2}&=\frac{y(1-y)}{x-\alpha},\\ yz&=x-\alpha,\end{aligned}\right\} \tag{8}$$

where $\alpha=r_0V^2/\gamma M$. Since the velocity is towards the star for $x<\alpha$, it follows that the rate of accretion is given by the amount of material entering the portion of III*b* lying between $x=\alpha$ and the star. This gives

$$\alpha\frac{\gamma M}{V^2}\cdot A=\alpha.2\pi\rho_\infty\frac{\gamma^2M^2}{V^3} \tag{9}$$

for the accretion rate. The determination of the parameter α is accordingly of great importance. The conditions on y are $y\to 1$ as $x\to\infty$ since the velocity must tend to V at great distances from the star, $y=0$ at $x=\alpha$ and y must be bounded in any finite range of x not including $x=0$. It should be noticed that the above equations break down near the star as transverse motion of the material will become important for values of $x<<1$. But even if the motion of the material could be followed up to the surface of the star it would still not be possible to impose any further condition on y, for it seems certain that near the star the inward velocity of material in III*b* will be greater than the velocity of sound so that no influence from the star could travel outwards through the material. Now the above conditions on y can be satisfied for any value of α, so that the steady state equations (8) are not sufficient to define a unique solution to the problem. In fact these conditions permit an infinity of solutions for every value of α. If it is assumed that y is monotonic with x (that is, the magnitude of y *increases* with decreasing x from $x=\alpha$ to the surface of the star, and y *increases* with increasing x from $x=\alpha$ to infinity), which would seem to be an essential physical requirement, then it can be shown from (6) that α must be >1. Thus for all values of $\alpha>1$ both the mathematical and physical requirements are satisfied and a unique solution cannot be determined. This means that the system may satisfy any one of a range of solutions, the particular solution satisfied at a given time being determined not only by the equations (8) and the boundary conditions but also by the perturbations experienced by the system. It is interesting to notice that all solutions for which $1<\alpha<2$ satisfy $(dy/dx)_{(x=\alpha)}=1/\alpha^2$.

4. In considering perturbations acting on the system discussed in section 3 we shall consider only disturbances that preserve axial symmetry. The forms of the differential equations are changed since A and V are functions of time in the disturbed state. The modification of (8) to include this change is simple, however, for we have

$$\frac{\partial m}{\partial t}+\frac{\partial}{\partial r}(mv)=A,\qquad \frac{\partial(mv)}{\partial t}+\frac{\partial}{\partial r}(mv^2)=AV, \tag{10}$$

in place of (7). Eliminating m gives the equation

$$\frac{\partial}{\partial t}\frac{A(V-v)}{(\partial v/\partial t+v\partial v/\partial r+\gamma M/r^2)}+\frac{\partial}{\partial r}\frac{Av(V-v)}{(\partial v/\partial t+v\partial v/\partial r+\gamma M/r^2)}=A. \tag{11}$$

Again introducing the non-dimensional variables x, y and writing $t=\gamma M\tau/V^3$ the equation (10) becomes, at the point $x=\alpha$,

$$\frac{\partial^2y}{\partial\tau^2}+\frac{3}{\alpha^2}\frac{\partial y}{\partial\tau}+\frac{2(2-\alpha)}{\alpha^4}y=\frac{1}{\alpha^2A}\frac{\partial A}{\partial\tau}+\frac{1}{\alpha^2}\left(\frac{\partial y}{\partial x}-\frac{1}{\alpha^2}\right), \tag{12}$$

where A represents the mass per unit length added to III*b* from II *at the point* $x=\alpha$. By restricting attention to disturbances for which $(\partial y/\partial x)_{x=\alpha}=1/\alpha^2$, it is easy to show from (11) that the system is stable only if $\alpha<2$. This result together with that of the previous section limits the value of α to the range $1<\alpha<2$. The value that α will have at any particular time will depend on the perturbations suffered by the system at previous

times. Accordingly to calculate a unique value of α it is necessary to consider the precise form of the disturbances experienced by the system. An investigation of this kind is attempted in the following section, where the steady state set up after a star passes into a uniform cloud of material possessing a plane boundary perpendicular to the direction of the relative velocity V is considered.

5. The boundary of the cloud is supposed plane when the star is at great distances, but will become distorted as the star approaches. The form of the boundary at any time is easily calculated. Thus consider the trajectory of a particle lying at the surface of the cloud with angular momentum pV about the star. The particle moves in a hyperbolic path which is given by the following equation until it crosses the accretion axis:—

$$\frac{p^2}{rl} = 1 + \cos\theta + \frac{p}{l}\sin\theta, \tag{13}$$

where $l = \gamma M/V^2$, and r, θ are polar co-ordinates with axis along the accretion axis (the equation of the path is normally referred to the line joining the star to the vertex of the hyperbolic path, but the present procedure of choosing the line through the star parallel to the accretion axis is more convenient here). The distance of the star from the vertex of the path is $r_1 = \sqrt{p^2+l^2} - l$, and if t_1 is the time at which the particle is at the vertex, then it is easy to show that

$$\left.\begin{aligned} V(t-t_1) &= \sqrt{r^2+2lr-p^2} + l\log\frac{r+l-\sqrt{r^2+2lr-p^2}}{\sqrt{l^2+p^2}} \qquad \text{if } t > t_1 \\ V(t-t_1) &= -\sqrt{r^2+2lr-p^2} - l\log\frac{r+l-\sqrt{r^2+2lr-p^2}}{\sqrt{l^2+p^2}} \qquad \text{if } t < t_1 \end{aligned}\right\} \tag{14}$$

If the particle were at distance r_0 at time $t_0 < t_1$, then it follows immediately from (14) that

$$V(t-t_0) = r_0 + \sqrt{r^2+2lr-p^2} + l\left\{1 + \log\frac{r+l-\sqrt{r^2+2lr-p^2}}{2r_0}\right\} \qquad t > t_1, \quad t_0 < t_1 \tag{15}$$

provided r_0 is very large. Now define t_s by means of the equation

$$V(t_s - t_0) = r_0 + l + l\log(l/2r_0), \tag{16}$$

so that $(t_s - t_0)$ is the time taken by a particle with $p = 0$ to fall to the star from distance r_0. Thus we have

$$V(t-t_s) = \sqrt{r^2+2lr-p^2} + l\log\frac{r+l-\sqrt{r^2+2lr-p^2}}{l}, \tag{17}$$

where $(t-t_s)$ is the difference between the times taken by a particle with angular momentum pV to fall to distance r, and by a particle with zero angular momentum to fall to the star, both particles starting from distance r_0 (which is very large). Accordingly, the form of the boundary of the cloud for $t > t_1$ (this condition is easily seen to be equivalent to $\tan\theta < p/l$, $r > l(1/\cos\theta - 1)$) is given by

$$\left.\begin{aligned} &\sqrt{r^2+2lr-p^2} + l\log\frac{r+l-\sqrt{r^2+2lr-p^2}}{l} = V(t-t_s), \\ &p^2 - rp\sin\theta - rl(1+\cos\theta) = 0. \end{aligned}\right\} \tag{18}$$

Solving for p from the second of these equations and substituting in the first equation gives, after some reduction,

$$V(t-t_s) = r\left(1 - \frac{\theta^2}{4}\right) + \text{higher terms in } \theta \tag{19}$$

for the equation of the boundary near $\theta = 0$.

It follows, therefore, that the point at which the boundary intersects the accretion axis moves with velocity V, and that the tangent plane to the boundary at this point is perpendicular to the axis. Behind this point collisions between the particles will tend to build up a column of the type discussed in section 3, this process being governed by equation (11), which can be written in the following non-dimensional form:—

$$\frac{\partial}{\partial\tau}\frac{1-y}{(\partial y/\partial\tau+y\partial y/\partial x+1/x^2)}+\frac{\partial}{\partial x}\frac{y(1-y)}{(\partial y/\partial\tau+y\,\partial y/\partial x+1/x^2)}=1, \tag{20}$$

where $\tau=V^3t/\gamma M$ and $x\leqslant\tau$. Equation (2) has the interesting property that, although a second-order equation, the single boundary condition that $y=1$ at $x=\tau$ is sufficient to determine a solution, since it can be seen from the nature of the problem that $y=1$, $x=\tau$ is a characteristic of the equation which is of the parabolic type.*

A solution of (20) of the form

$$y=1+\sum_{n=1}^{\infty}(\tau-x)^{n/2}f_n(x)$$

has been tried and the first few coefficients determined. The form of the power series is

$$y=1-\frac{1}{x}\sqrt{\frac{2}{3}}(\tau-x)-\frac{2}{21}\frac{\tau-x}{x}+\sqrt{\frac{2}{3}}\left\{\frac{25}{4\cdot147x}+\frac{1}{4x^2}\right\}(\tau-x)^{3/2}$$
$$+\left\{\frac{-23}{7\cdot441x}+\frac{1}{18x^2}\right\}(\tau-x)^2+\sqrt{\frac{2}{3}}\left\{\frac{1429}{16\cdot490\cdot441x}-\frac{2671}{8\cdot70\cdot441x^2}-\frac{431}{7\cdot480x^3}\right\}$$
$$(\tau-x)^{5/2}+\cdots \tag{21}$$

which converges so well for $x>1{\cdot}5$, $(\tau-x)<1$, that it seems sufficient to regard y as being given to good approximation by (21). In the following table the values of y corresponding to a series of values of $(\tau-x)$ are given for $x=2$ and $x=3$.

TABLE I

$\tau-x$	$x=2$	$x=3$
0·0	1	1
0·25	0·7827	0·8603
0·49	0·7144	0·8054
0·64	0·6773	0·7788
0·81	0·6417	0·7530
1·00	0·6078	0·7280
1·1025	0·5910	0·7156

Table I shows that y tends to a steady value as $(\tau-x)$ increases to a value near unity. If $y_{\lim}(x=2)$ and $y_{\lim}(x=3)$ denote these steady values of y, then a unique solution of the steady-state equation (8) can be obtained giving these values of y at $x=2$ and $x=3$ respectively. Thus for any given value of α there is one solution of (8) satisfying the condition $y=0$, $x=\alpha$; $y=y_{\lim}(x=2)$; $y\to1$ as $x\to\infty$. There is, however, only one value of α for which the solution so determined also satisfies the additional condition $y=y_{\lim}(x=3)$, $x=3$. This may be illustrated by the following numerical case, in which we put $y_{\lim}(x=2)=0{\cdot}5$, $y_{\lim}(x=3)=0{\cdot}6$. (It is important to notice that these particular numerical values of $y_{\lim}(x=2)$ and $y_{\lim}(x=3)$ have been chosen for the sake of definiteness, and that the present argument would be equally applicable if somewhat different values had been chosen.) The following three numerical solutions satisfy the conditions $y=0$, $x=\alpha$; $y=0{\cdot}5$, $x=2$; $y\to1$ as $x\to\infty$.

* Hadamard, *Problème de Cauchy*, Hermann et Cie, 1932.

TABLE II

x	$\alpha=1\cdot15$	$\alpha=1\cdot25$	$\alpha=1\cdot5$
2·0	0·500	0·500	0·500
2·1	0·508	0·515	0·541
2·2	0·5145	0·523	0·570
2·35	0·523	0·537	0·603
2·5	0·530	0·549	0·625
2·75	0·541	0·564	0·650
3·00	0·551	0·577	0·667

It is clear from Table II that a value of α close to 1·25 will give a solution satisfying the additional condition $y=0\cdot6$, $x=3$. Accordingly it is to be expected that a steady state will be set up giving an accretion rate close to

$$\frac{2\cdot5\pi\gamma^2M^2\rho_\infty}{V^3}.$$

It should be noticed that these considerations give an estimate for α only for the steady state set up after a special form of perturbation. The perturbation considered above is of a particularly violent character, and it would seem that for smaller perturbations α could hardly be less than the value obtained above.

6. *The Rate of Change of V.*—It is easy to show from (8) that for any steady-state solution $y\sim1-\log x/x$ as $x\to\infty$. Now in any steady state the mass of material crossing the section of III*b* at distance $x>\alpha$ is given by

$$A(x-\alpha)\frac{\gamma M}{V^2}=(x-\alpha).2\pi\rho_\infty\frac{\gamma^2M^2}{V^3}\text{ grams per sec.}$$

This material before approaching the star is moving with velocity V relative to the star, whilst after passing the star the velocity is modified to $V\left(1-\frac{\log x}{x}\right)$ provided $x>>\alpha$. Accordingly the momentum of the star relative to the cloud must be reduced by

$$2\pi\rho_\infty\frac{\gamma^2M^2}{V^2}(x-\alpha)\frac{\log x}{x}\sim2\pi\rho_\infty\frac{\gamma^2M^2}{V^2}\log x\text{ grams per sec.},$$

due to the gravitational action of the star on material striking the axis between α and $x>>\alpha$.

The equation of conservation of momentum is therefore given by

$$\frac{d}{dt}(MV)=-2\pi\rho_\infty\frac{\gamma^2M^2}{V^2}\log x. \tag{22}$$

The right-hand side of (22) $\to\infty$ as $x\to\infty$ on account of the term in $\log x$, but for astronomical reasons an upper limit can be assigned to x. The presence of neighbouring stars will introduce a cut-off effect analogous to that which arises in the calculation of the collision cross-section of a charged particle in an ionised gas. It is important to notice that since dV/dt depends only on $\log x$ the cut-off value of x need not be known at all accurately. Indeed it is clear that the average interstellar distance may be used as the cut-off value of $(x.\gamma M/V^2)$ without introducing any appreciable error in dV/dt. The average interstellar distance is about 3.10^{18} cm., and $\gamma M/V^2$ is of order 10^{16} cm. for stars of large mass and is of order 10^{13} cm. for stars of small mass. It follows, therefore, that $\log x$ lies between 5 and 15 for almost all stars. The equation (22) may be solved in conjunction with equation (9), which gives

$$\frac{dM}{dt}=\alpha.2\pi\rho_\infty\frac{\gamma^2M^2}{V^3}.$$

The solution for M, V at time t is given by

$$\frac{M}{M_0}=\left(\frac{V_0}{V}\right)^{\frac{a}{\log x}}, \qquad \frac{V}{V_0}=\left[1-\frac{6\pi\rho_\infty\gamma^2M_0}{V_0^3}\log x.t\right]^{\frac{1}{3}}, \tag{23}$$

where $V=V_0$, $M=M_0$ at time $t=0$.

From (23) it is seen that M increases significantly in a time given by

$$\frac{V_0^3}{6\pi\rho_\infty\gamma^2M_0}\log x.$$

This formula may be compared with the estimate of $V_0^3/(4\pi\rho_\infty\gamma^2M_0)$ given by Hoyle and Lyttleton.* It is seen from this comparison that the time-scale of the evolution of the stars due to accretion is shortened below earlier estimates by a factor of $1{\cdot}5\log x$, which, as seen above, is of order 10. This is an important result, since it would seem that the dynamical evolution of the stars can be achieved in a period of less than 10^{10} years. For example, if we take

$$V_0=5 \text{ km./sec.}, \qquad \rho_\infty=10^{-22} \text{ grm. per cm.}^3, \qquad M_0=2.10^{34} \text{ grm.}$$

as representing a typical star of Class B, then putting $\log x=10$, the time required for the mass of the star to increase by an appreciable factor is about $2{\cdot}5.10^9$ years.

The analysis given above neglects the temperature of the interstellar material. The observations of McKellar† indicate that the average thermal velocity u of interstellar hydrogen is of order 10^4 cm./sec. It will be realised that the analysis breaks down for material that passes the star at distances of the order of, or greater than, $\gamma M/u^2$, and a much more complicated investigation is required to describe the motion in this case since the effects of gas pressure can no longer be neglected. For $u=10^4$ cm./sec., $M=2.10^{33}$ grm., the distance $\gamma M/u^2$ is $1{\cdot}33.10^{18}$ cm., which is of the same order as the average interstellar distance. Thus for values of u of order 10^4 cm./sec. the effect of gas pressure on the motion of material at large distances from the star can have no appreciable effect on the value of dV/dt. Moreover, it is clear that since dV/dt depends only on $\log x$, the value of u could be appreciably in excess of 10^4 cm./sec.$^{-1}$ without any possibility of a change in the order of magnitude of dV/dt arising.

7. The process discussed in section 6 would appear to be of considerable importance, and it seems desirable to conclude the present paper with a few general remarks concerning this question. The problem of the resistance experienced by a gravitating body moving through a gas was considered by Jeffreys‡ who drew attention to the fact that the characteristic linear dimension occurring in the problem is $\gamma M/V^2$ and not the radius of the body (except of course when the mass of the body is so small that $\gamma M/V^2$ is comparable or less than the radius). Jeffreys regarded the drag due to the cloud as being capable of rounding up the initially elliptic orbits of the planets into approximately circular paths, and also of introducing a stabilising influence into the planetary system. This application of the process is analogous to the investigation of section 6, since the reduction of the velocities V of the stars relative to the interstellar material means that the peculiar motions of the stars are being continually reduced and that their paths are being rounded up into circular orbits.

The evolution of a star is dominated by a struggle between opposing forces. On the one hand we have the resistance of the interstellar material constantly tending to reduce the value of V, whilst on the other hand the perturbing force due to the changing gravitational field of the galaxy as a whole tends to increase the value of V (the exact nature of the changing gravitational field is a question that is outside the scope of the present

* *Proc. Cam. Phil. Soc.*, 35, 592, 1939.
† *P.A.S.P.*, 52, 187, 1940.
‡ *The Earth*, Cambridge, 1929.

paper, and the detailed discussion of this matter has to be left over for investigation in future work). The evolution of the star depends on the outcome of the struggle between these opposing tendencies. Thus if the changing gravitational field should become the dominating effect, then the orbit of the star in the galaxy will become elliptic and the star will cease to increase in mass on account of the large value of V that must arise; whilst if the opposing tendency should become predominant and the resistance of the interstellar material reduces V to small values, then the mass and luminosity of the star will increase to large values. Indeed it would appear that the question of whether the rate of accretion of hydrogen by a star is sufficient to balance the hydrogen destroyed by the energy generation in its interior is determined by the battle between the opposing forces mentioned above. Thus it would appear that the physical evolution of the stars is directly associated with the dynamical considerations governing the variation of V with time.

Although it is outside the scope of the present paper to discuss in any detail the problems of stellar dynamics, it is of interest that the interaction described above between the interstellar material and the stars may have an important bearing on stellar dynamics. Thus by assuming that an appreciable proportion of the mass of the galaxy is in the form of interstellar material (the existence of large tracts of interstellar hydrogen is known from observation; the observational data, however, are not at present sufficiently complete to enable an estimate to be given for the fraction of the mass of the galaxy in diffuse gaseous form), the whole subject of stellar dynamics would appear to be greatly simplified. This simplification is due to the fact that the interaction between the interstellar material and the stars enables the stellar system to be controlled by the diffuse gaseous material in the galaxy.

CHANGES IN γ CASSIOPEIÆ DURING THE PAST 100 YEARS

D. L. Edwards

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Summary

All available observations of γ Cassiopeiæ since 1830 have been collected and discussed, with the object of detecting any variations of magnitude or spectrum previous to the recent outburst.

There is strong evidence to show that such variations have occurred in the past. Magnitude changes have not been greater than about 0·3, and spectroscopic changes also seem usually to have been of small amplitude, but the combination of evidence from different sources leaves little doubt of the physical reality of the changes in many cases.

A periodicity is suggested for these changes, consisting of two maxima, separated by 4·30 years, in a main period of 10·67 years. The observations are shown to fit very closely into this scheme with the exception of the 1934·6 outburst, which seems to have been caused by some unusual event.

Introduction.—During the period from about 1870 to 1890 there was considerable speculation as to changes in the spectrum of γ Cassiopeiæ. Observers of those days were mostly convinced of the fact of such changes, but much doubt was felt later as to their reality—partly due to the inconsistencies of some of the early observations, but mainly to the lack of confirmation by photographic methods. The latter point is greatly stressed by R. H. Curtiss * in a useful summary of these early observations, but he says (referring to the line D_3): "On the other hand the very fact that this line was seen so frequently by visual observers indicates that, in that period, from 1872 to 1888, it must have been stronger, at least spasmodically, than at present". Also he sums up by saying that "notwithstanding the evidence tending to discredit the work of the early visual observers, it would seem that the time has not come to reject the results gathered by them". The striking changes observed in recent years destroy the argument from lack of photographic evidence, and make it reasonable to re-examine the early observations; they also give a clue as to the type of variations to be looked for. The possibility must be remembered, however, that the recent changes may be exceptional, or of larger amplitude than those of earlier changes.

An investigation of all the available records of early observations (both magnitude and spectroscopic) has been made, and supplemented by a re-discussion of some of the photographic observations, including a new series of measurements of the Sidmouth spectrum plates from 1921 onwards. Evidence in favour of variability in γ Cassiopeiæ was found to be unexpectedly strong on certain dates, and these dates were (also unexpectedly) found to fit into a definitely regular periodic scheme, consisting of a main 10·67-year period, with a second maximum in each period following 4·30 years after the first. This enabled further dates to be computed when similar variations might be expected, and additional evidence was found which, though sometimes slight, was in good agreement with these predicted dates. In presenting the results of this investigation in the present paper, all the evidence is collected and grouped together according to the different types of variation found, with indications given in each case of the expected dates of disturbance as computed from the suggested periodicity.

* *Pub. Mich. Obs.*, **2**, 1, 1915.

Magnitude Observations.—The first suggestion of variability in γ Cassiopeiæ was made by W. R. Birt *, who included it in a list of suspected stars. Although he refers again to this suspicion in a later paper †, no definite magnitudes are given, and neither he nor any other observer seems to have followed the matter up. The date of his observations, however (1830·7), is significant since it agrees almost exactly with one of the computed dates (1830·6). This may be simply coincidence, but is worth noting as an interesting fact.

Indirect evidence of the magnitudes of γ Cassiopeiæ may be deduced from observations of Argelander and others who used the stars β, γ and δ Cassiopeiæ as comparisons in observing variations of α Cassiopeiæ. By rearranging their original statements of step observations it is possible to eliminate the variable, α, and deduce the relation between γ and β or γ and δ (or both). For example, the two statements $\alpha 1 \beta$ and $\gamma 2 \alpha$ imply $\gamma 3 \beta$. The value of one step, and also the probable error of one observation, may be deduced from a series of $\beta - \delta$ relations (found in the same way), using the known magnitude difference from the HD values. Finally, from the known HD magnitudes of β or δ the actual magnitudes of γ can be found (subject to the errors of the original observations). In the example given above, if one step were $0^m{\cdot}09$, and $\beta = 2^m{\cdot}42$, the magnitude of γ would be 2·15.

The observations of Argelander ‡ cover the period 1840 July to 1845 October. The value of one step, found as above, was 0·09, which is in good agreement with the mean value (0·10) found at Harvard § for a much larger series of Argelander's observations. The probable error of one observation was $\pm 0{\cdot}06$. The resulting magnitudes of γ Cassiopeiæ were combined into groups covering time intervals of 2 to 3 months and averaging about 8 observations in each group, the mean value for each group being taken as the magnitude at the mean date. The results are shown in Fig. 1A, where the short vertical lines indicate the probable errors of each point. The points are connected by straight lines to assist the eye in following the trend of the changes, though a smooth curve following a mean course might be a truer representation of the facts. The arrows above the figure mark the computed dates of maximum, and dotted lines are used, where necessary, to suggest curves or extrapolations which would be a reasonable fit, both for the observed points and for the predicted dates.

A comparison of the total range of variation (0·20) with the average probable error ($\pm 0{\cdot}02$) suggests that in its main features the curve is valid, especially when the comparative regularity of the changes is considered, though some of the minor apparent changes are probably accidental. A fairly well-defined maximum is indicated at 1841·3, agreeing exactly with the computed date as shown by the arrow. The next computed date (1845·6) could easily fit in with the series, though observations are scanty at this period.

Fig. 1B shows the results obtained in a similar manner from observations by Oudemans ||, combined with a few later observations of Argelander. The value of one step for Oudemans was 0·10 and the probable error $\pm 0{\cdot}06$, so that the observations are of the same order of accuracy as Argelander's. The results show a maximum somewhere near the computed date of 1856·2, also a pronounced minimum about mid-way between this date and the nearest earlier computed maximum of 1851·9.

A short series of observations by Glasenapp ¶ in 1875–76 gave the results shown in Fig. 1C. The value of one step was 0·12, and the probable error of one observation $\pm 0{\cdot}07$. In spite of the short period covered by these observations there is a strongly marked increase in brightness towards the predicted maximum of 1877·6.

The earliest direct determinations of magnitude which could be found were those

* *M.N.*, **2**, 79, 1833.
† *M.N.*, **20**, 40, 1859.
‡ *Ast. Beob. Bonn.*, **7**, 491, 1869.
§ *H.A.*, **14**, 378, 1885.
|| *Verh. Kon. Akad. Amst.*, **3**, Zweijaehrige Beobachtungen der meisten jetzt bekannten veraenderlichen Sterne, p. 31, 1856.
¶ *Mitt. Pulk.*, **3**, 221, 1910.

of a long series of observations at Harvard * with meridian photometers. These observations have already been discussed by F. de Roy †, who pointed out the existence of a fairly well-marked maximum at about 1894·9, but considered that differences from the general mean on other dates were too small to confirm additional maxima. The original Harvard observations have here been rearranged in groups slightly more compact than those of de Roy. The mean date of each group has been computed to the nearest tenth of a year, and the final results are shown, with their probable errors, in Fig. 1E. This graphical representation shows very clearly the maximum at 1894·9 (agreeing closely with the computed maximum of 1894·6); it also brings out a smaller maximum exactly at the computed date of 1883·9. The latter is only of small amplitude (0·14), but this is four times the probable errors of surrounding points, and the variations are also supported by spectroscopic evidence (given later). Finally there is a regular upward

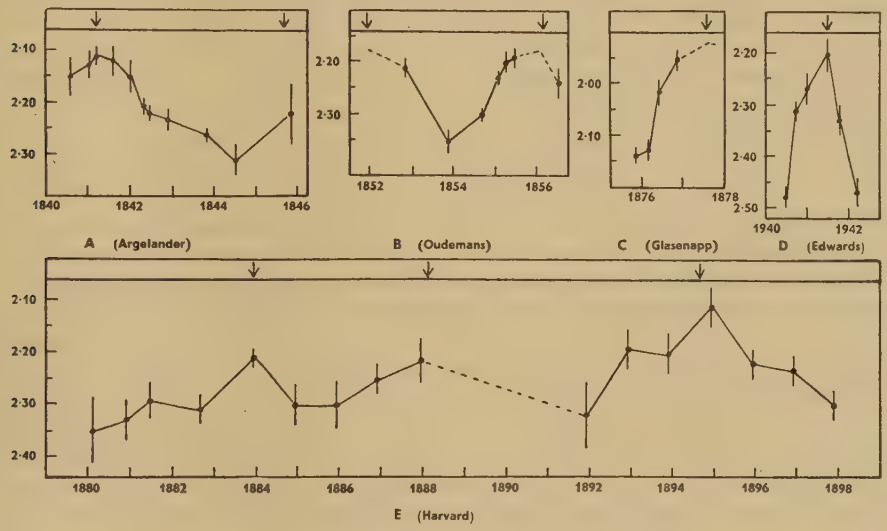

FIG. 1.—*Magnitude observations of γ Cassiopeiæ.*

trend in magnitude from 1886 to 1888 towards the computed maximum of 1888·2 which, for the same reasons, may also be real. The latest observations in the Harvard series (1900 September to 1901 December) cover too short a period to give much information beyond a mean magnitude at this epoch. If divided into two groups they give magnitudes of $2{\cdot}29 \pm {\cdot}02$ (1900 December) and $2{\cdot}26 \pm {\cdot}03$ (1901 November). The probable errors do not warrant an assumption of a real change between these dates, and the mean of all the observations gives $2{\cdot}28 \pm {\cdot}02$.

Observations in 1901 by Kopff ‡ of α Cassiopeiæ, treated in a similar way to those of Argelander, yield two points within the period covered by the last Harvard observations. They therefore form a useful comparison, as well as a slight check on the validity of the method of reduction. Kopff's observations show more scatter, with a probable error (of one observation) of $\pm 0{\cdot}14$. Some of this, however, is due to a real change, since this error is reduced to $\pm 0{\cdot}10$ if the observations are separated into two groups—a value nearly equal to that ($\pm 0{\cdot}09$) derived from the values of $\beta - \delta$. The mean magnitudes for the two groups are $2{\cdot}48 \pm {\cdot}04$ (1901 July) and $2{\cdot}30 \pm {\cdot}04$ (1901 September). The latter agrees very well with Harvard, but the mean of the two differs from Harvard by 0·11. This cannot be considered excessive, considering the limited material available and the fact that the range of values of individual observations are nearly the

* *H.A.*, 14, 23, 44–46, 1884–1903.
† *L'Astr.*, 50, 513, 1936.
‡ *Publ. Ap. Obs. Konigs.-Heidelberg*, 1, 189, 1902.

same (2·04 to 2·60 for Harvard and 2·00 to 2·66 for Kopff). It is suggestive that Kopff's magnitudes also show a brightening in 1901 similar to those of Harvard but of larger amount. The weighted mean of Harvard and Kopff is 2·33, which is slightly below the accepted average magnitude of 2·25 and suggests that γ Cassiopeiæ at this period was quiescent, or at a minimum. This agrees with the prediction, since the date lies between the two computed maxima at 1898·9 and 1905·3. It is just possible that the suspected increase in brightness marks the beginning of a rise towards the latter maximum.

No further magnitude observations have been found since 1901 until the recent well-observed outburst. It is not necessary to discuss these observations in detail, and it may simply be stated that there was a principal well-defined maximum in 1937 April–May, as shown by Huffer's * photoelectric observations, and by a carefully discussed series of visual observations by Rigollet and Oriano.† A second maximum is also shown (apart from minor fluctuations) in 1938 July. No later observations have reached Sidmouth, but a series of photovisual observations by the author ‡, if divided up into groups to smooth out small changes (see Fig. 1D), shows a sharp maximum at 1941 June, agreeing with the computed date of 1941·6.

The principal maximum, taken as 1937·3, and confirmed by numerous spectroscopic observations, has been used as the zero point in computing the dates of maxima given throughout this paper.

Visual Spectroscopic Observations.—Photographic observations of the spectrum in the last outburst indicate that the only sign of variability likely to have been noticeable to early visual observers is in the intensities of the emission lines. Finer details would have been beyond their power. Unfortunately the brightest *H* line (*Hα*) is the most liable to be affected by instrumental conditions since, as Curtiss says §: "It is near the edge of the region for which visual objectives are corrected". The very erratic and discordant observations of this line are evidently of little value. *Hβ* is a fainter line, more difficult to observe, and estimates of brightness may have been affected by personal, instrumental or atmospheric conditions. The D_3 line is the one most likely to give useful information since (as mentioned above) if it were seen at all this would indicate some sort of disturbance from the normal quiescent state. Finally, if repeated observations were made by an experienced observer, using an instrument with which he had previously seen bright lines, and he were unable to see any, this would be evidence of at least a decrease in intensity of these lines. If first place is given to observations of D_3, while those of *Hα* are entirely ignored and of other lines received with caution, then practically all the apparent inconsistencies of the early observations disappear. Useful summaries of this early work are given by Curtiss (*loc. cit.*) and J. N. Lockyer ||, from which further references may be obtained. If treated as above the main facts are as follows (emission lines only are referred to).

In 1863 Secchi saw D_3, as well as *H* lines. In 1866 he saw *Hβ*, also "others" (D_3 not specifically mentioned) too faint for measurement. In the following year both he and Huggins saw D_3 again. Vogel, in 1872, saw *Hβ* and a line "assumed to be D_3". These dates therefore indicate possible times of outbursts, though not necessarily exactly at the maximum phase. They agree reasonably well with dates computed in the present paper, viz. 1862·6, 1866·9, 1873·3.

No observations are available for intermediate dates, to test the fading out of D_3 between the above outbursts, but from 1874 onwards observations are more frequent. From 1874 to 1883 (exact dates not given) de Konkoly looked carefully for bright lines, but could not see any. Similar results were obtained by von Gothard in 1881–82, and by Kövesligethy in 1882. Maunder saw *Hβ* at various dates between 1880 and 1884, but usually faint or barely visible. The only other positive observation is of *Hβ* and

* *Ap. J.*, 89, 139, 1939. † *L'Astr.*, 52, 424, 1938. ‡ *M.N.*, 102, 246, 1942.
§ *Loc. cit.* || *Proc. Roy. Soc.*, 44, 43, 1888.

"477" by Copeland in 1877. There was, therefore, an apparently definite fading out of D_3 and a possible decrease in intensity of the *H* lines during the period 1874 to 1883. The only computed maximum during this period is at 1877·6, which coincides with Copeland's observations of a possible increase in emission. The evidence is scarcely sufficient to prove or disprove this prediction, though it receives additional support from Glasenapp's magnitudes given in the preceding section.

In 1883 there was an undoubted fresh brightening of D_3. This was first noticed by von Gothard, and later confirmed by de Konkoly, both of whom had been observing γ Cassiopeiæ regularly. By the end of the year D_3 was no longer visible. This brightening nearly agrees with the predicted date of 1883·9, and with the maximum in the Harvard magnitude curve described above.

For nearly five years after this D_3 apparently remained invisible, since no observations of it are recorded, while Copeland (1885 and 1887) and Maunder (1887 February–December) both specifically say it was not visible. It was seen again by Maunder in 1888, who described it as "faint". This observation fits the next computed date, 1888·2, and is supported by the trend of the Harvard magnitudes mentioned above. The subsequent fading of D_3 apparently continued until 1894. Keeler could find no trace of it in 1889, though the *H* lines (to *Hγ*) were still bright. Sir J. N. Lockyer's students record it as visible in September 1889, but failed to see it in October. Three photographs by Belopolsky in 1892 showed no trace of the line, and thus confirmed the fading. The next definite record of visibility was in 1894 October, by Lockyer's students, which fits very well the next computed date of 1894·6 and the Harvard magnitude curve.

After this date photography superseded the visual observations, and is dealt with in the next section. The results of the early visual work seem to give definite evidence of variability in the spectrum of appreciable, even if not large, amount. Every occasion when D_3 was observed corresponds to one of a regular scheme of computed dates, and every period when there was definite evidence of absence of D_3, or general fading of emission intensity, corresponds with intermediate dates. This conformity with a regular periodic scheme makes accidental, personal or instrumental errors an improbable explanation of the observations. The argument is still further strengthened by the independent coincidence of these particular dates with small observed magnitude changes.

The spectrum variations, however, are unlikely to have been on the same scale as the recent striking changes, since otherwise many more emission lines would almost certainly have been noticed. This is confirmed by the absence of any large scale magnitude changes in the available observations.

Photographic Spectrum Observations.—Coming next to photographic observations, the first and most obvious conclusion is that during the period up to 1933 there cannot have been any changes in the spectrum on a scale anything like that shown in the latest outburst. All observers agreed in describing the spectrum as constant (with the exception of possible small radial velocity changes), though it is shown later that there were some minor changes which had escaped notice. Later work suggests that if any small changes did occur the quantities to be examined with any hope of success are the intensities of emission lines, the *V*/*R* ratio for components of double emission lines, and possibly the radial velocity.

The only observations of intensity, until recently, have been visual estimates. These are on different arbitrary scales, and depend so much on personal judgment, instrumental and plate qualities, that it is impossible to combine results from different observers. Even those of one observer cannot be used unless they form a regular consistent series based on approximately uniform material and are sufficiently numerous to reduce the rather large accidental errors by taking means. The only series fulfilling these conditions is that of Curtiss (*loc. cit.*) based on Michigan plates taken between 1911 and 1914. In discussing his estimates of *Hβ*, *Hγ* and *Hδ* (see his Table VII),

Curtiss says that "there appears no evidence of appreciable change in the average intensities of these emission lines from year to year". It is interesting to note, however, that the figures in his table show a very slight increase in intensity from 1911 to 1914 for each of the lines, which apparently was considered to be too small to be genuine. As a check on this, the individual values of Curtiss for $H\beta$, $H\gamma$, $H\delta$ on each night have been combined into a mean H intensity. These intensities have then been taken in groups similar to those used by Curtiss, and the mean intensities found for each period. Probable errors of these final values were deduced from residuals from the means, and the results are as follows (number of observations in brackets):—

$$\begin{aligned} 1911: &\ 22{\cdot}2 \pm 0{\cdot}26\ (65), \\ 1912\text{–}13: &\ 22{\cdot}8 \pm 0{\cdot}37\ (55), \\ 1913\text{–}14: &\ 24{\cdot}1 \pm 0{\cdot}35\ (54). \end{aligned}$$

This shows up the regular, though slight, increase of intensity from 1911 to 1914. The first and last values differ from the mean by $-0{\cdot}8$ and $+1{\cdot}1$ respectively, differences which are approximately three times the average probable error ($\pm 0{\cdot}33$). The nearest computed date of maximum is 1915·9, and the small intensity increase suggested by the above figures might, if continued, reach a maximum at about this date. Unfortunately, no later observations of the same kind are available to confirm this.

The number of Balmer lines visible in emission is a useful criterion which is closely related to the intensities of the lines. Provided negatives are dense enough past the head of the Balmer series, and of good contrast, there should not be much error in the number of lines recorded. At Sidmouth a good series of plates is available from 1923 onwards, of fairly uniform character. The number of Balmer emission lines has been counted in each of these, and the mean values for each available month taken (the number of plates being from one to five in a month). The results up to 1932 are shown in Fig. 2A and indicate two fairly definite maxima near the two computed dates (shown by arrows) of 1926·6 and 1930·9.

The V/R ratio is the next quantity suggested as likely to show variability, but it must be noted that there is a phase difference between this and the other criteria used. From measurements made at Sidmouth (unpublished) of the later very pronounced variations it appears that when the magnitude, emission intensities, number of Balmer lines and radial velocity are at a maximum, the V/R ratio is near a point of inflection; it is decreasing, and not far from unity (i.e. $V = R$). The exact dates are thus not so accurately determinable as maximum or minimum values but may be found sufficiently well for the present purpose.

The earliest date for which a change in V/R has been noticed is 1892–96, in a series of plates taken at South Kensington. The changes were not noticed at the time, but a later re-examination of the negatives by W. J. S. Lockyer * showed definite evidence of variability during that period. The curves illustrating this change are not very well defined, but if the values for $H\gamma$ and $H\delta$ are combined and a new curve drawn on a rather more open scale (see Fig. 2C) the date of the inflexion point appears to be early in 1894. (It must be remembered that Lockyer's original curves are inverted, showing V/R increasing downwards.) This is in agreement with the computed date (1894·6) within the limits of observational errors.

Observations by Curtiss from 1911 to 1915 do not, according to him, support any hypothesis of variation at this date, the small apparent changes being attributed to uncertainties of observation. These would, if real variations, agree with the expected changes before the computed date of 1915·9, since they appear to show a very slight maximum of V/R in 1913, followed by a slight average decrease to the end of the series (1916 January), when $V/R = 1{\cdot}0$ approximately.

* *M.N.*, **93**, 619, 1933.

The first definite suggestion of variability in V/R was made by McLaughlin *, who states that "plates taken in 1926 and 1927 show $V/R = 1$ and later observations show the violet component slightly the weaker". In other words, V/R was decreasing and near unity in 1926–27, which fits very well the computed date of 1926·6.

More definite proof of this type of variability was given by W. J. S. Lockyer †, whose curves suggest the dates 1926·4 and 1930·2. The Sidmouth negatives used in this investigation have been re-examined in order to give independent verification of the change during this period, and to reduce the amount of personal error inherent in eye estimation. The V/R ratio has been estimated on an arbitrary scale different from that of Lockyer, and checked, when possible, by microphotometer measures with a method

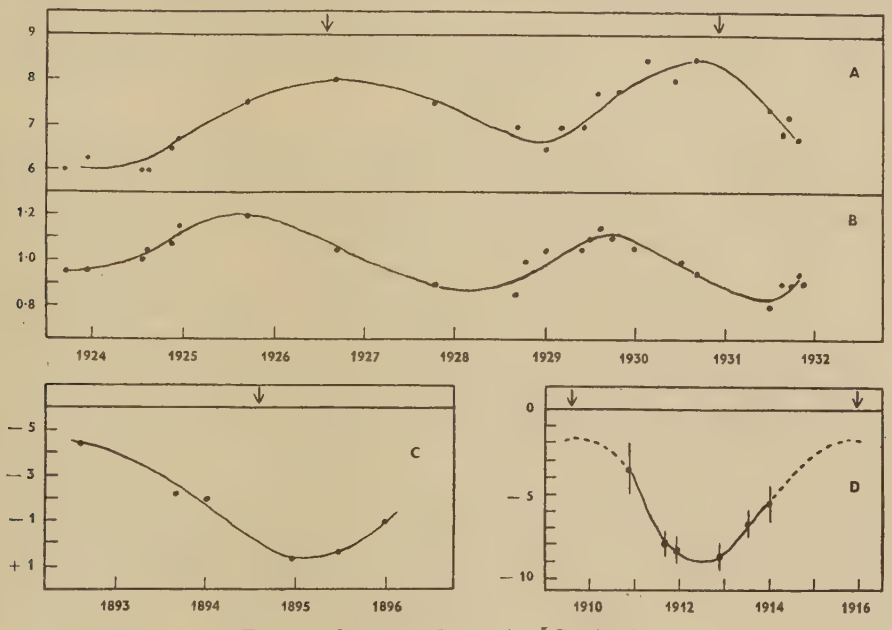

FIG. 2.—*Spectrum changes in* γ^1_2 *Cassiopeiæ.*

A. *Number of Balmer emission lines.*
B. *V/R for H emission lines.*
C. *V/R for H emission lines (South Kensington negatives measured by W. J. S. Lockyer).*
D. *Radial Velocity (Michigan and Lick Observatories).*

described by the author.‡ The results are shown in Fig. 2B. They agree fairly closely with those of Lockyer (allowing for the inversion of the curves), but give slightly later dates for the required phase (1926·7 and 1930·7). The means of the two (Lockyer and Edwards) are reasonably close to the computed dates of 1926·6 and 1930·9.

A study by Cleminshaw §, covering the years 1914–15, includes a diagram of the V/R changes (estimated by eye) during this period. Although on a small scale, there are suggestions of the expected decreases in 1916, 1920, 1926 and 1931, with the exception of $H\gamma$ on the first and of $H\delta$ on the last of these dates.

Radial Velocities.—The existence of radial velocity changes has been suspected, though without confirmation until recently, since the first announcement by Hartmann ‖ of variability. Curtiss (*loc. cit.*), from observations at Michigan between 1911 and 1917, showed conclusively that no variations of appreciable amplitude took place; also that short-period changes even of small amplitude could be excluded. He said, however,

* *Pub. Mich. Obs.*, 4, 177, 1932.
† *M.N.*, 93, 362, 1933.
‡ *Observatory*, 64, 136, 1941.
§ *Ap. J.*, 83, 495, 1936.
‖ *A.N.*, 173, 102, 1906.

that his observations "do leave open the possibility of a slow variation with a period and amplitude similar in character to that apparently suspected by Hartmann". The Michigan values have here been combined with a series from Lick *, covering a similar period, the total number of 77 observations being divided into six groups, for each of which the mean V was found. The results are shown in Fig. 2D. The probable error of one Michigan plate was ± 3 km./sec. That of Lick plates, computed from the residuals from the mean V (assumed constant), was found to be the same, and these values were used in computing probable errors of the mean velocities (shown by vertical lines). The diagram appears to show up the "slow variation", and suggests a minimum at about 1912·5—a date fitting well between the computed maxima (shown by arrows) of 1909·6 and 1915·9. Extrapolations of the curve at both ends could very reasonably show maxima at these two dates, and the two last Michigan values (not shown), giving $-6{\cdot}5 \pm 2{\cdot}2$ at 1917·1, would lie well on a continuation of the curve beyond the second maximum.

Cleminshaw †, from observations made in 1914–1934, does not regard velocity variations as definitely established, though he says that his curves "show very slight indications of a period of about 11 years, with minima near J.D. 2000 and J.D. 6000". This is interesting in view of the fact that the main period suggested in the present paper is 10·67 years. Cleminshaw's first minimum is supported by the large negative velocity derived at Yerkes from four plates in 1918, and both of them agree with computed minima of the present paper. It is also worth noting that the minor changes in his curves (attributed to accidental sources) give some very slight indications of maxima near each of the computed dates within the range, viz. 1915·9, 1920·2, 1926·6, 1930·9. These are especially noticeable in the $H\delta$ curves, but the evidence is too slight to be conclusive.

General Conclusions.—The foregoing analysis shows that, even before the recent outburst, γ Cassiopeiæ was subject to frequent variations both in spectrum and magnitude, though these changes were small compared with those observed after 1933. Although frequently very small (sometimes only slightly beyond the range of probable errors of observation) the reality of these changes is supported by the independent evidence, in many cases, from different observers, from magnitude and spectrum observations at the same epochs, and from observations of different characteristics in the spectrum. In several cases the changes are large enough to leave little doubt as to their reality without other evidence.

The genuine character of the variations is strengthened by the fact that all the suspected changes occur at dates which fall in a regular periodic series, thus making it improbable that they are due to random accidental errors. The existence of such a periodic recurrence was suspected when several well-supported dates of "outburst" had been found, but it was evident that no simple period would fit the observations. The best fit was found, by trial and error, to be given by a double maximum, with a separation of 4·30 years, recurring at intervals of 10·67 years. In other words, the maxima occurred regularly at intervals of 4·30 and 6·37 years alternately. Using these figures, with the well-determined date of 1937·3 as a starting-point, the series of computed dates of maxima were determined. These are the dates referred to throughout this paper, and indicated by arrows in the figures. The 100 years covered by this investigation includes twenty such dates, for three of which (1898·9, 1905·3, 1909·6) no suitable observations could be traced, in confirmation or otherwise. At each of the other seventeen dates observations have been found suggesting changes in magnitude or spectrum or both, and even when of only small range they indicate a maximum phase at or near the predicted dates. For intermediate times the available observations suggest a minimum or quiescent stage. (Occasionally an upward or downward trend, towards or away from a maximum, is noticeable, when near one of the predicted dates.) On the other hand, no observations have been found (with one principal exception, mentioned below) which suggest changes

* *Pub. Lick Obs.*, **16**, 11, 1928. † *Ap. J.*, **83**, 495, 1936.

of this type at any other dates than those computed. The evidence is thus strongly in favour of a periodic scheme such as that suggested.

The principal exception, mentioned above, is the outburst which reached a maximum at 1934·6 approximately. Although of a well-marked character, and considerably larger than previous variations, it does not fit in with the suggested scheme, and was apparently due to some exceptional cause different from that underlying the normal changes. As it appeared shortly before the normal date of outburst in 1937, and was of considerable magnitude, it possibly accounted for the unprecedented range of variations shown in the latter outburst. It is interesting to note that there was also a minor maximum (the only other exceptions to the main series) shown by slight changes in magnitude and spectrum in 1936·9, which is exactly 4·3 years later—the same period as that between the two maxima in the main series.

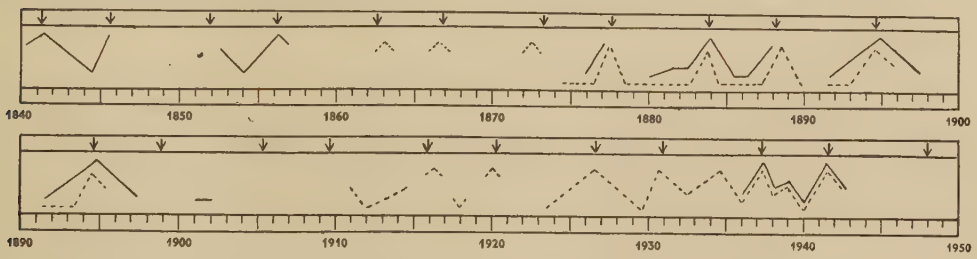

Fig. 3.—*Relation of observations to suggested periodicity.*
(*Full lines = magnitude observations; dotted lines = spectroscopic observations.*)

The results are summarised in Fig. 3, which represents symbolically the observations which have been discussed. Continuous lines refer to magnitudes and dotted lines to spectroscopic observations. When the observations suggest a maximum phase, *i.e.* a maximum of magnitude, line intensity (including visual observations of D_3) or radial velocity, or the corresponding phase of V/R, then they are plotted near the top of the strip. If they suggest a minimum or quiescent stage they are plotted near the bottom, and observations showing an increase or decrease are represented by lines inclined in the corresponding direction. Where no observations were available a blank space has been left. No scale is used, and no attempt made to indicate the amplitudes of variations, the whole being purely diagrammatic with the intention of showing the coincidences between observation and prediction. The computed dates of maximum are indicated by arrows as before. The first observation, by Birt, is not included since it is too vague in character, but as already mentioned it would fit closely the computed date of 1830·6.

Norman Lockyer Observatory,
Sidmouth:
1944 *August* 22.

A NOTE ON THE PULSATION THEORY OF CEPHEID VARIABLES

P. L. Bhatnagar and D. S. Kothari

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Summary

The problem of the anharmonic pulsations for the homogeneous model (and also the Roche model) pulsating in the fundamental mode is discussed. The ratio of specific heats is assumed to be 5/3. The results are compared with those of Rosseland.

In his George Darwin Lecture * on the Pulsation Theory of Cepheid Variables Professor Rosseland has developed the general theory of anharmonic pulsations and has applied it to a homogeneous star pulsating in the fundamental mode, which case is amenable to a simple mathematical treatment. In this case Rosseland finds that for an appreciable skewness in the velocity-time curve, the semi-amplitude of pulsation nearly approaches one-fourth the stellar radius (the observed amplitude is about one-tenth the radius). However, this and other results obtained by Rosseland are not inherent in the model, but arise on account of an approximation introduced in the investigation. The work discussed in the present note is free from this approximation (the value of the ratio of the specific heats is assumed to be 5/3). As it happens, the exact treatment is simpler than that of Rosseland. In the following, as far as practicable, we use his notation.

1. Let r, p, ρ, T and g denote radius vector, pressure, density, temperature and gravity of an element in the Lagrangian sense and a, p_0, ρ_0, T_0 and g_0 denote these quantities at a given initial time t_0. For a homogeneous star pulsating in the fundamental mode we write for the displacement at any point a

$$r - a = a\eta q, \tag{1}$$

where η is a constant and q is a function of time only. In the sequel we shall identify t_0 with the instant when the pulsation velocity is maximum, it being directed outwards.

We note that for the homogeneous model

$$p_0 = \tfrac{1}{2}\rho_0 \frac{GM}{R}\left(1 - \frac{a^2}{R^2}\right) \tag{2}$$

and

$$g_0 = GMa/R^3, \tag{3}$$

where M is the mass of the star and R its radius at time t_0. We shall call R the equilibrium radius.

The kinetic energy $W_1(t)$ of radial oscillation is

$$W_1 = \tfrac{1}{2}\int_0^R 4\pi a^2 \rho_0 da\ a^2\eta^2\dot{q}^2 = \tfrac{3}{10}MR^2\eta^2\dot{q}^2 \tag{4}$$

and the work $W_2(t)$ done against gravitation, measured from the state at time t_0, is

$$W_2 = -\frac{3}{5}\frac{GM^2}{R}\left[\frac{1}{1+q\eta} - 1\right] = \frac{3}{5}\frac{GM^2}{R}\frac{q\eta}{1+q\eta}. \tag{5}$$

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* *M.N.*, 103, 233, 1943.

Further, the increase in the thermal energy $W_3(t)$, measured again from the state at time t_0, is

$$W_3=\int_0^R C_v(T-T_0)4\pi a^2\rho_0 da,$$

where C_v is the constant volume specific heat per unit mass. Substituting the perfect gas equation

$$p=C_v(\gamma-1)\rho T$$

and the adiabatic relation

$$p/p_0=(\rho/\rho_0)^\gamma=\frac{1}{(1+q\eta)^{3\gamma}},$$

where γ is the ratio of the specific heats, we obtain using (2)

$$W_3=\frac{1}{5(\gamma-1)}\frac{GM^2}{R}\left[\frac{1}{(1+q\eta)^{3\gamma-3}}-1\right]. \tag{6}$$

Adding (4), (5) and (6) we have for the total energy W, which does not vary with time,

$$W=\frac{3}{5}MR^2\left[\tfrac{1}{2}\eta^2\dot{q}^2+\frac{MG}{R^3}-\frac{q\eta}{1+q\eta}+\frac{1}{3(\gamma-1)}\frac{MG}{R^3}\left\{\frac{1}{(1+q\eta)^{3\gamma-3}}-1\right\}\right]. \tag{7}$$

Expanding to the third power of η we get

$$W=\tfrac{3}{5}MR^2\eta^2[\tfrac{1}{2}\dot{q}^2+\tfrac{1}{2}\sigma_1^2q^2-\tfrac{1}{2}\sigma_1^2(\gamma+\tfrac{1}{3})\eta q^3], \tag{8}$$

and hence we get

$$\ddot{q}+\sigma_1^2q=\tfrac{1}{2}(3\gamma+1)\sigma_1^2\eta q^2;\qquad \sigma_1^2=(3\gamma-4)\frac{MG}{R^3}, \tag{9}$$

which is the equation of motion to the first power of η and has been given by Rosseland. His discussion of the homogeneous model is based on it.

It is convenient to introduce a new variable $x(t)$, where Rx represents the displacement at the surface of the star at time t. We have from (1)

$$x=\eta q. \tag{10}$$

Introducing this variable and substituting $\gamma=\frac{5}{3}$, we have for W the expression

$$W=\frac{3}{5}MR^2\left[\frac{1}{2}\dot{x}^2+\frac{MG}{R^3}\frac{x}{1+x}+\frac{1}{2}\frac{MG}{R^3}\left\{\frac{1}{(1+x)^2}-1\right\}\right]. \tag{11}$$

2. Introducing x and substituting $\gamma=\frac{5}{3}$ in Rosseland's equation we have

$$\ddot{x}=-\sigma_1^2x+3\sigma_1^2x^2;\qquad \sigma_1^2=\frac{MG}{R^3}, \tag{12}$$

which on integration gives

$$\dot{x}^2=-\sigma_1^2x^2+2\sigma_1^2x^3+c, \tag{13}$$

c being a constant of integration.

Let x_1 and x_2 represent respectively the "outside" and the "inside" amplitudes of oscillation, *i.e.* the stellar radius oscillates between the limits $R(1+x_1)$ and $R(1-x_2)$. The semi-amplitude K of oscillation is defined by

$$K=\frac{x_1+x_2}{2}.$$

It follows from (12) that for oscillatory motion x_1 cannot exceed $\frac{1}{3}$, for otherwise $\ddot{x}$

would become positive. For $x_1=\frac{1}{3}$, equation (13) gives $x_2=\frac{1}{6}$, and hence the limiting value for the semi-amplitude K is

$$K=\tfrac{1}{4}.$$

It is to be observed that the existence of these upper limits for x_1, x_2 and K have no physical basis; they arise merely because in the equation of motion terms higher than the second power of x have been ignored. According to the exact equation (11) x_1 can be infinitely large.

Equation (12) can be solved, as has been done by Rosseland, in terms of elliptic functions in the usual way. Let P denote the period of oscillation, t_1 the part of the period during which the radius of the star exceeds the equilibrium value R (expansion phase) and t_2 the part of the period for which the radius is less than R (compression phase),

$$P=t_1+t_2.$$

We shall call $\delta=t_1/t_2$ the skewness of oscillation. In the limit of vanishing amplitude $\delta\to 1$ and $P\to P_0$, where

$$P_0=\frac{2\pi}{\sigma_1}=2\pi\left(\frac{R^3}{MG}\right)^{\frac{1}{2}}.$$

As $K\to\frac{1}{4}$, $x_1\to\frac{1}{3}$, $x_2\to\frac{1}{6}$ and P/P_0 and δ both tend to infinity.

Table I(a) shows values for x_1, x_2, K, P/P_0 and δ obtained by solving (12) with the help of tables of the elliptic functions. From the well-known properties of the elliptic functions the asymptotic expression for P/P_0 is readily obtained,

$$\frac{P}{P_0}\sim\frac{1}{\pi}\log_e\frac{1}{2-8K}\quad\text{as}\quad K\to\tfrac{1}{4}. \tag{14}$$

TABLE I(a)

x_1	x_2	K	P/P_0	δ
0	0	0	1	1
0·0025	0·00249	0·0025	1·00	1·01
0·027	0·026	0·0265	1·00	1·07
0·074	0·064	0·069	1·02	1·20
0·163	0·120	0·142	1·09	1·50
0·220	0·145	0·183	1·19	1·78
0·279	0·161	0·220	1·39	2·30
0·318	0·166	0·242	1·74	3·15
0·330	0·167	0·248	2·00	3·77
$\frac{1}{3}$	$\frac{1}{6}$	$\frac{1}{4}$	∞	∞

TABLE I(b)

x_1	x_2	K	P/P_0	δ
0	0	0	1	1
0·0025	0·00249	0·0025	1·00	1·01
0·027	0·026	0·0265	1·00	1·07
0·074	0·064	0·069	1·01	1·19
0·163	0·123	0·143	1·03	1·43
0·220	0·153	0·187	1·05	1·60
0·279	0·179	0·229	1·08	1·76
0·318	0·194	0·256	1·095	1·84
0·330	0·199	0·265	1·10	1·91
0·333	0·200	0·267	1·10	1·92
∞	0·500	∞	∞	∞

3. We have so far discussed Rosseland's approximate equation of motion. We now take up the exact equation (11) which we rewrite as

$$\dot{\xi}^2=\sigma_1^2\left[-A+\frac{2}{\xi}-\frac{1}{\xi^2}\right] \tag{15}$$

$$\xi=1+x$$

and

$$A=1-\frac{10}{3}\frac{R}{GM^2}W=1-\frac{W}{W_0},$$

where $(-W_0)$ represents the total energy for the statical configuration of radius R. The parameter A determines the amplitude of oscillation. If as before $x_1=\xi_1-1$ and

$x_2 = 1 - \xi_2$ denote respectively the "outside" and the "inside" amplitude of oscillation, then, putting $\dot{\xi} = 0$ in (15), we have

$$\xi_1 = \frac{1}{A}\{1 + (1 - A)^{\frac{1}{2}}\}, \qquad \xi_2 = \frac{1}{A}\{1 - (1 - A)^{\frac{1}{2}}\},$$

$$x_2 = x_1/(1 + 2x_1), \tag{16}$$

and solving for A in terms of x_1 and x_2,

$$A = \frac{1 + 2x_1}{(1 + x_1)^2} = \frac{1 - 2x_2}{(1 - x_2)^2}.$$

The period of oscillation P, and t_1 and t_2 (the parts of the period for which the radius of the star is greater and less than R respectively) are immediately obtained from (15). We have

$$\frac{P}{P_0} = \frac{1}{\pi}\int_{\xi_2}^{\xi_1} f(\xi)d\xi, \qquad t_1 = \frac{2}{\sigma_1}\int_1^{\xi_1} f(\xi)d\xi, \qquad t_2 = \frac{2}{\sigma_1}\int_{\xi_2}^{1} f(\xi)d\xi, \qquad f(\xi) = \frac{\xi}{(-A\xi^2 + 2\xi - 1)^{\frac{1}{2}}},$$

and after integration we get

$$\frac{P}{P_0} = \frac{(1 + x_1)^3}{(1 + 2x_1)^{3/2}} = \frac{(1 - x_2)^3}{(1 - 2x_2)^{3/2}};$$

$$t_1 = \frac{2}{\sigma_1}\left[\frac{x_1(1 + x_1)}{1 + 2x_1} + \frac{(1 + x_1)^3}{(1 + 2x_1)^{3/2}}\left\{\frac{\pi}{2} + \sin^{-1}\frac{x_1}{1 + x_1}\right\}\right],$$

$$t_2 = \frac{2}{\sigma_1}\left[\frac{x_2(1 - x_2)}{1 - 2x_2} + \frac{(1 - x_2)^3}{(1 - 2x_2)^{3/2}}\left\{\frac{\pi}{2} + \sin^{-1}\frac{x_2}{1 - x_2}\right\}\right],$$

$$t_1 + t_2 = P,$$

x_1 and x_2 being related according to (16).

It is to be noted that $\delta = t_1/t_2$ depends only on x_1 or x_2 and is independent of M and R. Unlike the case of Rosseland, we have obtained here exact expressions for the quantities of physical interest. To facilitate comparison between his results and those obtained here Table I(*b*) has been added.

It will be noticed that for a given skewness of the oscillation the semi-amplitude K must be much larger than that given by Rosseland's approximate treatment. Thus, for example, for $\delta \sim 5$, $K \sim 0{\cdot}8$, which is more than three times the Rosseland value.

4. It may be remarked that the foregoing discussion is equally applicable to the Roche model, which consists of a central point-mass surrounded by a massless atmosphere and, as is easily verified, it is only necessary to write $(5/2)M$ in place of M in the equations given above—M is the mass of the star which in the Roche model is concentrated at the centre. Thus it follows that for the Roche and the homogeneous models, both having the same mean density, for a given skewness of oscillation, *i.e.* value of δ, the semi-amplitude K and so also x_1 and x_2 have the same values, but period for the Roche model is $(2/5)^{\frac{1}{2}}$ times that for the homogeneous model. This smallness of the period for the Roche model compared with that for the homogeneous model for all values of the amplitude provides an illustration of what appears to be a general result *, that, for the stars having the same mean density and the same ratio of specific heats, the period is largest for the homogeneous model and decreases as we pass to models in which the mass is more concentrated towards the centre.

5. From the preceding discussion the conclusion is obvious that the theory of

* T. E. Sterne, *M.N.*, 97, 582, 1937. (Also P. L. Bhatnagar, *Proc. Nat. Inst.*, in press.)

anharmonic oscillations (for $\gamma = \frac{5}{3}$) cannot account for the observed skewness in the velocity-time curve of the Cepheid Variables—the observed skewness demands a semi-amplitude almost equal to R, whereas the observed value is about 0·1 R—unless the value of γ is assumed to be considerably larger than 5/3. A rough calculation indicates that if the observed skewness is to arise for a semi-amplitude of 0·1 R, γ is comparable to 10.

Department of Physics,
University of Delhi:
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